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THE QUARTERLY REVIEW of BIOLOGY

SOIL GENESIS AND THE PEDOGENIC FACTORS

By ROBERT L. CROCKER

University of California, Berkeley, California

INTRODUCTION

THE idea that the study of the soil is an independent branch of natural philosophy seems to have been widely, even if not absolutely, accepted. It has been given the name pedology. Pedology, according to one definition, is "the study of the phenomenon presented to us by the soil body in its natural position" (Joffe, 1936). Yet pedology has had difficulties in establishing itself as an entirely separate branch of natural science, perhaps nowhere better illustrated than in the failure of attempts to define the soil as an independent natural body (Hilgard, 1921; de Sigmond, 1938; Kellogg, 1941; etc.). It remains a borderline science with diffuse boundaries that encroach upon the other natural science disciplines. It is not surprising, then, in view of inherent difficulties in defining the soil as an independent natural body, that some pedologists have used a different approach. Robinson (1936) declined a definition and merely indicated the range of interest of the pedologist, while Jenny's (1941) statement that the soil is "those portions of the solid crust of the earth the properties of which vary with the soil-forming factors," in expressing an identity, introduces the idea of soil-forming agents. This definition has been paraphrased in terms of the soil-forming processes in the recently published *Soil Survey Manual* (1951). Such definitions take us back to the founder of pedology, Dokuchaev, who said, "soils are surface deposits of mineral and organic formations, always more or less coloured by humus . . . always the result of the mutual action of the following agents. living and dead organisms (plants and animals), maternal rock formations, climate, and the relief of the location." Soil is a natural body but not an independent natural body. It is a part of the ecosystem (Tansley, 1935).

The soil profile is generally recognized as the unit of study in pedology. It is the summation of the soil properties, as they vary with distance from the surface, to an undefined depth, but deep enough to reach unaltered, or little altered, parent material. The soil profile is the result of the action and interaction of the pedogenic factors. The study of soil formation, therefore, is based upon the characteristics of soil profiles. Soil characteristics frequently exhibit marked variability with depth, and may have maximal or minimal expression at certain horizons. At other times, little change may occur in the profile. Soils whose morphological profile characteristics show sharp variations, that is, marked horizon development are widely regarded by soil scientists as well developed, or mature, soils. They are said by many to be in a state of equilibrium, or near equilibrium with their environment. Soils with poorly developed and variable profile characteristics are said to be immature, or juvenile. Much data and many observations support these generalizations, although they are not to be regarded as universal laws.

Scope of Review

Two general approaches are discernible in the literature of studies on soil genesis. One approach

leads to consideration of soil dependence upon environmental factors. The other is concerned more with the actual processes and mechanisms by which unaltered parent material has been modified into soil.

It is proposed in this article to consider the recent literature coming to the author's notice, and concerned with the first of these approaches, namely, factors in pedogenesis. Further, in order to restrict the review to reasonable limits, only contributions relating, or having particular significance to the theoretical aspects of the factors in soil formation are to be considered. An especial objective is to examine the theoretical basis of attempts to provide a more quantitative background for pedology. These revolve mainly about the philosophy outlined in Jenny's book, *Factors in Soil Formation* (1941). The theoretical basis to the approach in this book will be outlined and discussed, both briefly in relation to its historical development and more fully in relation to basic ideas, and to any achievements. This seems worthwhile inasmuch as no very comprehensive review of the theoretical basis of this approach has been published in English, yet its impact has already become widespread. To fulfil this objective it has also been found necessary to discuss in some detail both the confusing and difficult vegetation factor, and polygenesis and soil evolution, before proceeding to a final evaluation of what has become known as the functional, factorial approach.

THE PEDOGENIC FACTORS

Historical

Hilgard in the United States and Dokuchaev in Russia first emphasized that a number of influences in addition to parent material were important in soil formation. Earlier concepts had stressed the significance of the mineral material from which the soil apparently formed, rather than either other pedogenic factors, or the properties of the complete soil body. It is proposed here for several reasons to discuss only Dokuchaev's viewpoint. Firstly, Dokuchaev had a balance and perspective which, for its completeness, was remarkable; secondly, for one reason or another, he has had more general recognition; and thirdly, he has been frequently quoted, and misquoted, in reference to Jenny's book. It is not intended to minimize Hilgard's independent, and in some ways, wider contribution.

In his studies on the Russian chernozem, Dokuchaev saw the fallacies of the earlier concepts and in a series of papers published between 1877 and 1900, and unsurpassed in the clarity of their pedological reasoning, he established that climate, subsoil (parent material), vegetation, fauna, man, age of the land surface, and relief are the significant pedogenic factors. Although I have been unable to check the claim, it is also said that he formalized the relationships beween the soil and the pedogenic factors in the equation

$$\text{Soil} = f\ (\text{climate, organisms, subsoils, age})$$

Thus, in something less than twenty years—all the significant ideas, in fact, were established by 1883—not only was the prospective independent discipline of pedology launched, but more was achieved by Dokuchaev and his students (and by Hilgard), in laying the foundations and fundamental bases of the genetic aspects of the new discipline, than by all pedologists in the following half century.

The question arises whether Dokuchaev regarded the factors as independent or interdependent variables. According to Stephens (1947), he believed they were not independent of each other. Joffe (1936) has written that "Dokuchaev considered all the factors of soil formation as a complex," and Neustreuv (1927) that "although Dokuchaev had established such factors of soil formation as climate, parent rock, relief, vegetation and age of the country, he still, in the first place, emphasized that it is their interaction which leads to definite conditions of soil formation." In those of Dokuchaev's papers available to me, I can find no verification that it was his view that the interaction of the pedogenic factors is especially significant, nor in the claim (Stephens, 1947) that he believed they were not independent of each other. On the other hand, numerous statements of Dokuchaev make it clear that with the exception of organisms, he considered that the factors could operate independently. This does not mean that he ever regarded the soil as the result of a single factor, for he did not. Although admitting that the relative significance of the factors varies, he always stressed that the soil is the expression of all of them (1881, 1883). That Dokuchaev believed most pedogenic factors could be independent of each other is apparent in the following quotation (1881): ". . . a difference in the soil-creating factors (all of them, or only one), causes also a difference in the resulting soils. Furthermore, if two or more soils differ among themselves in their secondary properties . . . but are entirely uniform, in the kind, for instance, of

their maternal rocks and their age, then the reason for the difference of these soils has to be found in the unlikeness of the other two soil-creating factors: climate and vegetation. The same reasoning stands for a vice-versa situation." Perhaps the best proof of Dokuchaev's belief of the possible independence of the soil-forming factors rests in the evidence and manner of reasoning by which he established his soil formers, so well illustrated in his publication of 1881, and in the pedologic classic, *Russian Chernozem* (1883). His method was in large part one of isolating the variables. For example, he showed that the alignment of the chernozem belt in Russia is independent of the parent material and that chernozem occurs on the diverse lithology of geological formations belonging to all epochs from the Devonian to the Pleistocene. At the same time, differences in these parent materials, where factors of age, relief, and vegetation did not vary significantly, did result in variations within the chernozem. It was because the chernozem belt was aligned parallel to the isorhythms of temperature and precipitation that Dokuchaev regarded climate, in this case, as of dominating importance.

At all events, more than half a century of soil work added no further soil-forming factors to the list proposed by Dokuchaev. Slow progress was made by his successors in determining the significance of the soil-forming factors, either as to their independent effects, or their interactions as soil formers. Indeed, the development of the zonal concepts of soil formation, apparently first elaborated by Sibertsev (Neustreuv, 1927; Joffe, 1936), a student of Dokuchaev, overemphasized the predominant role of climate to the serious neglect of the other factors.

Because between the equator and the poles climates (and vegetation) appear arranged in horizontal zones, the idea was developed by Dokuchaev's students that soils must necessarily be arranged in latitudinal belts. As Mikhailooskaia (1939) said, "Latitudinal zonality was made into a dogma, and it dominates unreservedly soil science for half a century." Dokuchaev himself, in his later years, was drawn into the zonal enthusiasm of his disciples sufficiently to say (1899): "Since nature, taken as a whole is zonal, as demonstrated by its different realms of plants, animals, minerals—then it would be but natural, even unavoidable, that the soils as results of the united action of the mentioned soil-creating agents, could not be otherwise but zonal." In view of his other writings, the inclusion of "minerals" here is puzzling and seems only attributable either to Dokuchaev's age or the zonal fever. His support was only limited, however, and he went on to indicate some of the limitations. ". . . the outlined picture of the horizontal soil zones is only a scheme of a law, but in order to express itself in an ideal form the elevation of the different parts of the earth above the sea-level would be expected to be uniform . . . all the waters (sweet as well as sea-water) would be arranged uniformly and symmetrically relative to the continents all over the globe. But, fortunately . . . such a deadening mathematical uniformity does not exist in nature." Mikhailooskaia (1939) quoted a report of Dokuchaev to the Transcaucasian Committee for the Estimation of Soil Values, which is equally clear on the point.

The ideas of Dokuchaev were almost completely hidden from West European and American pedologists until the publication of Glinka's book in German (1914), and were not widely known until Marbut translated the book from German to English (1927). Unfortunately, Glinka was an ardent follower of the current zonal concepts, and Dokuchaev's ideas and philosophy were only partially, and very inadequately, presented [Marbut appears to have given some additional emphasis to climate in his translation.] Had Dokuchaev's papers instead of Glinka's book found their way into English at that time, the development of pedology in Europe, Australia, and especially the United States might have been along somewhat different lines.

Dokuchaev's factors were not entirely forgotten or obscured, however, and were beginning to be considered once more, and developed anew, about the time that Jenny (1941) published his book, *Factors in Soil Formation*. Evidence of this return to a wider view of the pedogenic factors is apparent, for example, in Jenny's earlier publications (1930, 1935, etc.) and in the 1939 Yearbook of the United States Department of Agriculture, *Soils and Men*, and is also clearly demonstrated by Rozjanetz (1939) and Mikhailooskaia (1939).

TOWARDS A MORE QUANTITATIVE PEDOLOGY: THE SOIL AS A SYSTEM

Jenny (1941) proposed a new approach to soil formation, and especially to clarifying the part played by the various factors. It was at once both an application and a significant extension of the ideas of Dokuchaev. Jenny regards the soil as an

open dynamic system which, in turn, is part of a wider system made up of the soil and its environment—something closely approaching, but not identical with, Tansley's (1935) ecosystem. The soil and the environment can, in fact, be looked upon as coupled systems where many properties pass continuously from one to the other. The soil system is characterized by certain properties, the soil properties. These properties are capable of both qualitative and quantitative expression and their totality defines the system. Because certain of these properties are functionally related, that is, covarying, it is not necessary to determine all the properties to fix the system. The smallest number of properties capable of determining the system are those which can be made to vary independently of each other—the independent variables. Jenny considers, by analogy with simple chemical systems, that in the dynamic soil system if we know the conditioning variables (the soil climate, soil relief, and soil organisms, and in addition the time involved and the initial state of the system, the soil parent material), we have all the independent soil-forming factors necessary to define a soil. He expresses the relationship between any soil property (and hence the soil, which is a collection of properties), in the equation

$$s = f(cl', o', r', p, t) \qquad (1)$$

where cl', o', r' represent soil climate, soil organisms, and soil relief, and p and t, parent material and time, respectively.

As soils and their environment form coupled systems and the soil properties cl', o', and r' are continuous with and functionally related to climate, organisms, and relief in the environment, Jenny replaces the soil properties, cl', o', and r' by cl, o, and r. Thus in the final equation, designated as "the *fundamental* equation of soil forming factors," cl, o, and r represent environmental climate, organisms, and relief:

$$s = f(cl, o, r, p, t, \cdots)$$

Jenny adds that, in selecting these as the independent variables of the soil system he does "not assert that these factors never enter functional relationships among themselves," but places emphasis on the fact that the soil formers *may* vary independently and *may* be obtained in a great variety of constellations, either in nature or under experimental conditions. He recognized also, by inserting dots, that all the soil formers may not be included in the fundamental equation and that additional ones may have to be added later.

Dokuchaev (1898) regarded the soil as the "central core" in a wider system, a wider "natural science discipline." The pedogenic factors he recognized are also identical with those of Jenny, and the idea that they define the soil is inherent, more or less, in the views he expressed in his doctorate dissertation (quoted by Joffe, 1936, and to be found with variations elsewhere, e.g., Dokuchaev, 1881, 1899), namely, that "if we know the factors of soil formation we are able to state in advance what the soil must be." He was even able to do this on a limited scale. From relationships he had already established between the pedogenic factors and soils Dokuchaev predicted the kind of soil he might meet on a forthcoming exploration of Transcaucasia, and later declared (1899), "I am happy to be able to declare now, after the accomplishment of this exploration that the reality, in its flawless perfection, exceeded all my theoretical expectations." In view of the possibilities for combination between the soil-formers, he was indeed fortunate that the restricted relationships of his experience were sufficient to project into another region. They certainly would not have held if projected sufficiently far afield to embrace wider combinations of the factors. It has never been possible, except in very special cases, for pedologists to achieve successful predeterminations because sufficient necessary basic relationships between soils and the factors of their formation have never been accumulated.

Jenny, in giving the early Russian ideas more precision, has also suggested a method by which the type of basic information essential for Dokuchaev's advance determination might be obtained. At the same time, in introducing his definitions of the soil as a system, certain restrictions have also been imposed which severely limit its applicability in practice.

Jenny stresses that the factors are not necessarily to be looked upon as "forming" in the sense that there is any causal relationship, they are merely "defining" factors—they define the soil system.

Possible Solution of the Fundamental Equation, and Soil Properties as Functions and Sequences of the Pedogenic Factors

The approach developed by Jenny is, to this stage, in broad conformity with the earlier, more

generally-expressed ideas of Dokuchaev, but is much more precise. Jenny goes on to make an entirely new suggestion, namely, that the fundamental equation might be solved, and the quantitative solution of the soil-forming-factor equation is "one of the most fundamental problems of theoretical soil science." The introduction of quantitative pedology seems to be the next logical step from Dokuchaev; it is surprising it was so long delayed.

Many of the difficulties of solving the fundamental equation were apparent to Jenny. For example, he appreciated the difficulty of assigning a numerical value to all the variables, especially organisms and parent material. He was, however, obviously sanguine of ultimate success. He undertook, as a start, "to assemble the known correlations between soil properties and soil-forming factors, and, as far as possible, express them as functions." A large part of his book is taken up with this task. In this way he believed it should be possible to discover how soil properties vary with changes in the pedogenic factors, even if inherent difficulties result only in approximations. From the fundamental equation an equation of partial derivatives was obtained:

$$ds = \left(\frac{\delta s}{\delta cl}\right)_{o,r,p,t} \cdot dcl + \left(\frac{\delta s}{\delta o}\right)_{cl,r,p,t} \cdot do + \left(\frac{\delta s}{\delta r}\right)_{cl,o,p,t} \cdot dr + \left(\frac{\delta s}{\delta p}\right)_{cl,o,r,t} \cdot dp + \left(\frac{\delta s}{\delta t}\right)_{cl,o,p,r} \cdot dt \qquad (2)$$

It follows that in the soil system, as defined, the degree of change in any particular soil property depends upon the total changes in the soil-forming factors, while the magnitude of the partial derivatives in the above equation would be true indices of the relative importance of the individual factors.

By keeping all factors but one constant, and following the changes in soil properties in relation to the single variant, it should be possible to discover the role played by each of the soil-forming factors. That is, the aim is to establish the following functions:

$$s = f(\text{climate})_{o,\,r,\,p,\,t,\,\cdots} \qquad (3)$$

$$s = f(\text{organisms})_{cl,\,r,\,p,\,t,\,\cdots} \qquad (4)$$

$$s = f(\text{topography})_{cl,\,o,\,p,\,t,\,\cdots} \qquad (5)$$

$$s = f(\text{parent material})_{cl,\,o,\,r,\,t,\,\cdots} \qquad (6)$$

$$s = f(\text{time})_{cl,\,o,\,r,\,p,\,\cdots} \qquad (7)$$

The accumulation of these functions is looked upon as a first step in quantitative pedology. In a later paper (Jenny, 1946), the above are called climofunctions, biofunctions, topofunctions, lithofunctions, and chronofunctions, and are elevated to "the five canonical functions of pedology."

The clarity with which Jenny has reasoned to establish a means of determining the role of the individual factors, in view of the pre-existing confusion on the subject (if we except Dokuchaev), is one of the greater successes of his book. The approach was becoming apparent, in a more restricted way, to others. For example, Rozjanetz (1939), a Russian, said, "... a correct estimation of the part played by the climate factor in soil formation, and the determination of its characteristic manifestation in soils, are possible only under conditions of some uniformity of the other soil forming factors ..."; and again, "in the study of the soil-forming process in space, it is necessary first of all to establish a sort of 'pure lines' of soils according to the soil-forming strata," that is, parent material. The master himself, as usual, had demonstrated prophetic clarity on the subject. When referring to the role of climate, he said, "... the vast majority of samples, to be taken in places in between the points outlined in my tables, shall prove in their humus contents, and in their secondary elements in general, to fit exactly in between these points. Of course, for such an achievement only normal soils, deriving from similar rock formations, have to be sampled" (Dokuchaev, 1881). [Elsewhere normal soil is defined thus: "only the chernozem of absolutely level regions can be regarded as normal." Vegetation in this case is held not to vary significantly.] The prediction of a perfect climofunction! And who could still say that Dokuchaev did not believe that pedogenic factors could vary independently?

When translated from the abstract to field conditions, the difficulty of keeping four factors constant while permitting the fifth to change is considerable. Some variation cannot be avoided and perfect functions are probably not attainable. However, if the magnitude of the variability of one factor is very great compared with the variability of the remaining factors, then equations should be possible which indicate all the significant general trends. A similar result is possible, even if some factors vary considerably, providing they are of little importance in determining differences in soil properties; that is, the magnitudes of their partial

derivatives are very small. Jenny (1946) also discusses the criteria for approximations, and demonstrates how, in practice, ensembles of soil properties, that is, soils, can be arranged in sequences in accordance with the variation of the soil-forming factors so as to demonstrate their genetic relationships. This can be done without assigning numerical values to the factors. More restricted, but similar, arrangements of sequences have been made by Bushnell (1942) and Butler (1950).

Because, in the functional approach, the factors must be independent of each other, the definitions of individual factors have especial significance. To define the factors in such a way that they were independent of each other was Jenny's great task, and, although some of his definitions may still require modification, and cannot be said to have been very widely accepted, they are a large measure of his achievement.

Parent material is defined as the initial state of the soil system, or the state of the soil system at soil formation time zero. It follows that, as the soil system is completely defined by the pedogenic factors, a change in one of them, as presently recognized, other than time—that is, in climate, organisms, parent material, or relief—will change the system. The ultimate state of the old system then becomes the new parent material at the initiation of the new system. Climate is defined as environmental climate, and both a moisture and temperature component are recognized. The biotic factor (organisms) is also divided into a number of subfactors, to be treated separately, namely: microorganisms, man, other fauna, and vegetation. The microorganism factor is considered as constant, because "nearly all soils at some time or another receive samples of most of the soil microorganisms," while the vegetation factor (better termed the flora factor), "may be obtained, as a first approximation, by enumerating all plant species growing within the area." Thus Jenny defines his independent biotic factor, not as the particular ecologic group or association, or frequency of organisms established and flourishing on any site, but as the "potential" occupiers. The topographic (relief) factor is defined as "the configuration of the land surface." While the groundwater table is considered under the topographic factor for convenience in theoretical discussions, it is deemed preferable to treat it as an independent variable or soil-forming factor, "because it can be made to vary independently of *cl*, *o*, *r*, *p* and *t*."

The foregoing brief summary gives the essential theoretical considerations behind Jenny's approach to soil genesis, as outlined in his book *Factors in Soil Formation*, and in his subsequent publication (1946). Special problems arise, in both the practical and theoretical aspects, as a result of the limitations inherent in the definitions of the individual factors which are such an essential part of his soil system. For the present let us confine ourselves to the general approach. Unfortunately, even here the reviewer's task is not simple because contributions to theoretical pedology are exceedingly rare, despite the stimulation and satisfaction inherent in the subject.

Reaction to the Theoretical Basis of the Functional, Factorial Approach

The only theoretical discussions of Jenny's ideas coming to my notice have been from Russian, British, and Australian soil scientists. The silence of American pedologists, in view of their numbers, is surprising. There is little doubt, however, that *Factors in Soil Formation* has had a tremendous effect in clarifying, and directing, American pedological thinking, and has greatly encouraged a reevaluation of the concepts of Dokuchaev. This is well illustrated by a comparison of the approach in any recent pedological or soil survey literature (e.g., Smith, Allaway, and Reicken, 1950; Kellogg and Nygard, 1951; *Soil Survey Manual*, 1951) with that of ten or fifteen years ago. Indeed, the book has had a much greater response than reaction to it on the theoretic level would suggest.

The most detailed discussion of Jenny's approach to soils has been that of Kovda (1946), who has hailed it as "an important event in the development of the modern genetical soil science," and who, while deploring the omission of "available Russian literature," concluded that, while raising new problems it "has solved some of the most important questions of theoretical pedology." Along with sentiments like the foregoing, there are also numerous criticisms of what is called "a series of debatable and unclear deductions," and the "mechanical character of Jenny's approach." The criticisms, however, are concerned with irrelevant details and barely touch the theoretical basis; indeed, it is apparent that Kovda has misunderstood both the significance of Jenny's definition of the soil as a system and his objectives. Gerassimov (1947) has made a more scholarly appraisal. He appreciates to some degree the importance of the

definitions, although he believes the analogies are oversimplified, and the reasoning contradictory. To Gerassimov, Jenny's philosophical discussion about the soil-forming factors and causality is quite contradictory, and he believes that in placing environmental climate, organisms, and relief in the fundamental equation, the role of soil-forming factors as environmental causes is admitted. He objects to Jenny's considering the factors relief and, especially, time, in the same way as the other factors. Nikiforoff (1942) and Stephens (1947, 1951) were also critical about the treatment of the time factor. Stephens believes that a better mathematical procedure is to equate soil as an integral of the soil-forming factors against time, and enlarging an equation published by Wilde (1946), he suggested the modified equation, namely,

$$s = \int f(c, o, r, w, p)\, dt \qquad (8)$$

in which c, o, r, w, and p can have both dependent and independent status, and w represents the water-table. Robinson (1949) has also pointed to the special part of time, which "in itself cannot have any effect but intervenes as governing the amount or extent of operation of other factors." There is probably much to be said for Stephen's equation as a better representation of the soil body. Equation (8) is however of very little scientific value, inasmuch as solution of it would be quite impracticable in view of the number of coexistent variables, and it does not permit a breakdown into simpler functions. Yet Jenny's treatment of the time factor in his theoretical consideration remains rather unsatisfactory to many. One difficulty with his partial derivative equation is that $\frac{\partial s}{\partial t}$ can be zero only for special cases of equilibrium in the physico-chemical sense, and for steady state conditions (maturity) in soils, while dt is never zero since time is continuous. McLaren (personal communication) has recently suggested that the theoretical misunderstandings associated with the time factor might be overcome by substituting two equations for the fundamental equation, namely

$$s = f(cl, o, r, p, \cdots) \qquad (9)$$

and

$$s = f(t) \qquad (10)$$

These equations can be used to give essentially Jenny's "five canonical functions." From equation (9) we can obtain

$$s_t = f(cl)_{o,r,p,\ldots} \qquad (11)$$

$$s_t = f(o)_{cl,r,p,\ldots} \qquad (12)$$

$$s_t = f(r)_{o,p,cl,\ldots} \qquad (13)$$

$$s_t = f(p)_{o,r,cl,\ldots} \qquad (14)$$

and from equation (10),

$$s_{o,r,p,cl,\ldots} = f(t) \qquad (15)$$

The substitution of the two equations recognizes the especial role of the time factor and that cl, o, r, and p are functions of time (equating (9) and (10)). At the same time the equations are explicit and susceptible of operation in providing functions in terms of the individual soil-forming factors. Equations (11) to (14), inclusive, indicate that measurements of soil properties, namely s_t, are to be made simultaneously on soils of the same age. It is to be noted that *this is what Jenny has done in practice.* Because equations (11) to (15), inclusive, do not in any case diverge from Jenny's functional, factorial usage, the latter's notation will be retained throughout this review. McLaren's divergence from Jenny is largely one concerning symbolism, based upon the idea that equations (3) to (6), inclusive, suggest that time can be held constant—an impossibility.

Both Gerassimov (1946) and Stephens (1947) were concerned about the fact that certain of the factors, for example, organisms, can have both dependent and independent aspects. Discussing the dependent-independent aspects Gerassimov said, "we have also to keep in mind that all the other soil-forming factors (climate, vegetation and parent material) never appear as being absolutely independent variables. They are linked to each other by the chain of their functional relationship, and, although being the real reason for the origin of different soil types, they are at the same time the results of other natural phenomena (causes)." Gerassimov, Lobovoi, and Fridland (1950) have claimed that Jenny's functional approach "turns out practically to be impossible—and methodically to be untrue. . . . All the factors of soil formation as a whole are found to be in close interdependence with one another, variations of each of them causes variations of the others." The last thought in this quotation is, of course, quite indefensible.

Gerassimov (1946) pointed out that *Factors in Soil Formation* does not contain a single universal solution of the "fundamental equation," and be-

lieved this to be because of "the absolute impossibility to express, by means of simple linear functions, a correlation between all the factors . . . due to their extremely complicated, variegated, dynamically and qualitatively differing-among-themselves natures. . . . The mathematical apparatus applied by H. Jenny to this effect is too primitive." Stephens also pointed to the danger of oversimplification. Retaining both a dependent and independent status for time in addition to climate, organisms, relief, water table (following Jenny's suggestion), and parent-material, Stephens derived a set of simultaneous equations of partial differentials from $s = f(c, o, r, w, p, t)$. By considering in rotation one factor independent and the remainder dependent, he has concluded that the soil might be represented by the sum of the simultaneous equations, namely:

$$\frac{\delta s}{\delta c}\left(\frac{\delta c}{\delta o}+\frac{\delta c}{\delta r}+k_3+\frac{\delta c}{\delta t}\right)+\frac{\delta s}{\delta o}\left(\frac{\delta o}{\delta c}+1+\frac{\delta o}{\delta r}+\frac{\delta o}{\delta w}+\frac{\delta o}{\delta p}+\frac{\delta o}{\delta t}\right)+\frac{\delta s}{\delta r}\left(\frac{\delta r}{\delta c}+k_2+\frac{\delta r}{\delta p}+\frac{\delta r}{\delta t}\right)+\frac{\delta s}{\delta w}\left(\frac{\delta w}{\delta c}+\frac{\delta w}{\delta o}+\frac{\delta w}{\delta r}+1+\frac{\delta w}{\delta p}+\frac{\delta w}{\delta t}\right)+\frac{\delta s}{\delta p}\left(\frac{\delta p}{\delta c}+\frac{\delta p}{\delta o}+\frac{\delta p}{\delta r}+\frac{\delta p}{\delta w}+1+\frac{\delta p}{\delta t}\right)+\frac{\delta s}{\delta t}(k_1)$$

In selecting his soil-forming factors, Jenny chose those capable of acting independently and he stressed, as already pointed out, that it was not necessary that these factors never enter functional relationships among themselves. It is clear, however, that he never intended them to be used in any functional way, except as independent variables. Stephens, therefore, is using the factors quite differently than with the defined restrictions. It appears that Stephens would agree with Gerassimov that Jenny's definitions are "fully insufficient to suit a universal formula, like the fundamental equation." However, while the charge that Jenny has oversimplified the situation has merit, Stephens has greatly overemphasized the complexity. In a later paper Stephens (1951) has formulated equations in terms of soil-forming processes. One might question how this can achieve anything unless the soil-forming factors are fully understood first.

Much of the disagreement with and misunderstanding of Jenny by Stephens is due to different objectives. Both are interested in a more exact pedology. Stephens has the approach of the soil surveyor or geographer anxious to include each and every geographical complex of soils in a scheme demonstrating their comprehensive genetic interrelationships. In fact, one aim of his paper is to present a diagrammatic method of demonstrating these relationships. Jenny also appreciates this need, and his sequences, which have advantages over Stephens' scheme, is an attempt to meet it. Bushnell (1945) has appreciated this and points out that Jenny's sequences put catenas in their logical place in relation to all other considerations, a judgment well supported by the recent work of Hallsworth, Costin, Gibbons, and Robertson (1952). On the other hand, Jenny's first aim is to accumulate the basic quantitative data on the relationship of soil properties and the independent factors in their development. By using only independent factors, and by selecting special cases in nature, he believes it is possible to establish functions which precisely express this relationship. The method is analogous in many ways to designing a controlled experiment; indeed, Butler (1950), in using a similar but less rigorous approach to studying soil genesis, has incorrectly defined it as "experimental pedology." Both Gerassimov and Stephens seem to have overlooked the significance of Jenny's definitions. Only the independent features can be used in his equation; the soil-forming factors are independent in his system by definition.

Robinson (1949) believes that the inherent difficulties of giving quantitative expression to the soil-forming factors are the main limitations of the functional approach. This is a practical difficulty which Jenny himself not only always recognized but stressed. It is not a theoretical difficulty and might diminish or be ultimately resolved in the future.

Factor Substitution in the Fundamental Equation

There are several theoretical aspects of Jenny's argument and reasoning that do not seem to have been discussed in any papers coming to the reviewer's notice. Some of them might with advantage be introduced here. One concerns the substitution of environmental climate, organisms, and relief in the fundamental equation, for the independent soil properties of soil climate, soil organisms, and soil relief in the initial equation. It is easy to see why this was done, for without it

the practical difficulties of measuring these soil properties would make the determination of functions very difficult. But why were soil climate, soil relief, and soil organisms included in the initial equation? Are they independent properties? Furthermore, the substitution is confusing when considered in relation to the independence of factors as defined for use in obtaining functions, or sequences. The substitution is made on the following reasons:

$$s = f(cl', o', r', p, t)$$

cl'(soil climate) $= f_1$(environmental climate, cl)

Therefore $s = f_2(cl, o', r', p, t)$.

Similar substitutions are held possible for o' and r'.

Now soil climate is obviously also a function of the actual vegetation on the site, as well as environmental climate. It is therefore legitimate, using similar reasoning, to include vegetation in the equation, thus

$$s = f(cl, v, o', r', p, t)$$

This would be at variance with Jenny's treatment, which considers vegetation dependent and flora an approximation of the independent biotic factor.

To the reviewer Jenny's inclusion of the properties of soil climate, soil organisms, and soil relief in his initial equation appears as an unhappy attempt to persuade that the soil system can be isolated from the ecosystem. For example, what precisely is meant by soil relief? The aspects of relief affecting soil formation are regional, since length, degree, and direction of slope and area are involved. Theoretically, a soil can be looked upon as having area approaching zero; its relief properties, per se, can hardly be a factor in soil formation. Furthermore, soil organisms, although a property of the soil, are largely, if not wholly, dependent on the other factors, and flora, for example, has regional implications. Indeed it seems unlikely that using properties like cl', o', and r', and p and t, we could define the state of the soil system. There seems little advantage therefore in not going to a direct consideration of "environmental," or "outward" factors, such as climate, organisms, and relief. Soil climate, organisms and relief are dependent aspects of the ecosystem.

The Definition of Parent Material

Jenny was conscious of the fact that a change in one of the independent variables other than time would change the soil system, and that a soil might be the expression of more than one combination of soil-forming factors. But he appears to have underestimated the significance of the fact. He attempted to overcome the difficulties it introduced by his ingenious definition of parent material, namely the initial state of the soil system —the state at soil formation time zero, t_o. There appears to have been no adequate discussion anywhere of the significance and implications of this definition, which is a critical one for the system. An attempt will be made here, therefore, to analyze some of the limitations inherent in it.

Two factors are concerned in the definition, parent material and time. Upon every occasion that the system changes, through a significant variation in any of the environmental factors, soil formation starts again—that is, we have a new parent material, and, of course, a new time zero. This places severe restrictions on where Jenny's approach might be used, because it follows that we can only obtain functions for soils which are the product of the current complex of climate, organisms, relief, and parent material, or where the changes have been insignificant, that is, wherever the definite integrals of the factors are very small.

Perhaps the greatest problem in practice when seeking a soil function like

$$S = f(cl)_{o,p,r,t}$$

or,

$$S = f(t)_{o,p,cl,r}$$

is to determine time zero, and the parent materials (state of the system) at that stage. At present there is no satisfactory method of doing this except in young soils, but it does not seem impossible, providing the change in systems is sharp and distinctive, that in certain cases it might ultimately be resolved. The change in a variable (or variables), however, must be relatively sudden if this is to be achieved, for slow variation results in continuous change of the system, a continuous change in both parent material, and in soil formation time zero, which becomes elusively migratory. Slow change is probably frequently associated with climatic changes. Similar difficulties result from other slow changes, such as the more or less continuous change in parent material from losses (e.g., degrading surfaces), or accessions (e.g., aggrading surfaces; loess, wind-borne oceanic salts, some minerals mobilized by plants) from outside the system, unless these can be considered functionally related to the remaining factors. Some aspects of the biotic factor

likewise may exhibit slow change, and hence create similar difficulties.

THE VEGETATION FACTOR

Plants are only part of the biotic factor in soil genesis, but probably the most important part. Jenny's definition of the independent plant factor is scarcely less ingenious than that of the parent material. It also raises problems, for the separation of the dependent and independent aspects of vegetation is especially difficult. This definition is to be discussed along with a more detailed consideration of the role of vegetation, which becomes necessary both to clarify a somewhat confusing discussion by Jenny and to attempt to estimate its worth. It also serves to draw attention to the type of assumptions that are necessary in practice with the functional, factorial approach.

General Significance of Plants in Pedogenesis

Soils and vegetation are so intimately associated in the ecosystem, and each is so peculiarly a part of the other's environment, that it is not surprising that attempts at isolating them in nature, for convenience of study, have been attended with numerous problems and only limited success. The parallelism between some of the concepts in plant ecology and pedology is striking evidence of this close relationship of soils and plants. Such parallelism is well demonstrated, for example, in the theories concerning the developmental aspects (time functions) of vegetations and soils. There are the ideas of autogenic succession and allogenic succession in ecology, and of autogenic evolution and evolution resulting from changes in external factors in soils; the concepts of development toward vegetation equilibrium, the *climax* state, and toward soil equilibrium, the maturity state; the single climatic climax, or so-called "monoclimatic climax" theory (so fervently sponsored by Clements) in plant ecology, and the development of the zonal concepts in pedology. The monoclimatic climax and zonal theories, in their extreme developments, had an interesting cross-fertilizing effect upon each other, each drawing support, especially plant ecology, from unbalanced generalizations in the other. An example of a more productive cross-fertilization of dynamic ecology and pedology is furnished by Braun-Blanquet and Jenny (1926) and by Pearsall (1952).

The close relationship between vegetation and soils is apparent in a voluminous ecologic literature drawing attention to, or stressing, the fact. Major (1951) has recently discussed this relationship in an interesting and important paper proposing a scheme for considering quantitatively the interrelations between vegetation and specific environmental factors. This scheme is essentially an application of Jenny's functional soil approach to plant ecology. Major stresses the fact that "vegetation and soil develop concomitantly" in response to the same factors, and that in general no universal correlation can be found between a vegetation and a soil property. If soils and vegetation develop simultaneously in response to the same factors, obviously there must always be a correlation between them. The fact that "no universal correlation" can be found is merely a logical result of the great possibilities for variation in both combinations of the independent environmental factors, and the time-states of the systems. A universal correlation would not be expected. Major believes that ecologists should work with the concept of soil parent material rather than with the soil (edaphic) factor. This has merit, but would be generally valid only if all soils and ecosystems were monogenetic. The fact is, however, that they are not. Plants, as individuals, have a life which, although variable, is short by comparison with the duration of development of most soils. This is too significant to neglect. Indeed, unless the current plants have materially changed the soil during their lifetime, it is quite sound to seek regional correlations between the soil body and vegetation. It is only in certain cases that soil parent material will prove a more useful concept to ecologists than the edaphic factor, but these cases might well be of great importance.

Many soil properties, such as the nitrogen and carbon profiles, are so directly related to plant residues that an especial significance for vegetation in soil formation has long seemed obvious. A considerable literature concerned with the physicochemical characteristics of plant residues is indicative of numerous possibilities. An interesting example is the recent attempt of Pearsall (1952) to assess the relative significance of vegetation and certain other factors in determining the pH of natural soils. Perhaps the most striking indication of the importance of the plant factor in soil genesis is that of the frequent occurrence of the broader soil classes (The Great Soil Groups) in zones. Within these zones the same general profile characters may occur over a wide range of parent mate-

rials and some variation of topography. The so-called zonal concepts of soil formation which developed from observation of such occurrences, although frequently mentioning vegetation as a contributory factor, placed the main emphasis upon climate (Glinka, 1914; Marbut, 1928; etc.). It was largely because of their strong climatic bias, or eccentricity, that the zonal concepts became somewhat sterile. If emphasis was to be placed upon any single factor, and such is undesirable, it would have been better placed upon plants, for vegetation was the only factor (within the restricted range of parent materials, and with the elimination of relief by considering only "normal" surfaces), which was both always concerned and had truly zonal qualities. The different soil zones, it is true, had different climates, but both within and between the zones were climatic gradients, variable in slope, but nevertheless continuously varying. A limited number of zones cut this gradient into sections. Climatic differences between points in different zones might easily be less marked than within zones. Such soil zones are, in general, essentially the result of the selective effect of the climatic and physico-chemical environment upon organisms with specific ranges of tolerance, and in so far as the ranges of tolerance for biotypes, either directly or indirectly playing a significant part in soil development, overlap, also by competition. Thus where clearcut soil zones of this nature occur, and it must be emphasized here that their occurrence is far from universal, they primarily result within variations, in parent material, relief, and the time factor, which are insignificant, pedologically, by comparison with the natural plant groups sifted by climate, and by competition. As the ranges of tolerance and competitive capacities are inherent qualities of the biotypes, practically undetermined by current climatic variability, plants are undoubtedly of foremost importance in determining such soil zonality.

The importance of vegetation as a factor in soil formation, early recognized by Dokuchaev, has been greatly elaborated, as will be apparent from later sections of this review, by subsequent Russian pedologists, especially by Williams and his contemporaries. The modern Russian view regards organic cycles in general, and those associated with plants in particular, not only as important contributors to soil-formation, but as determinants of all the essential and unique properties of soils. Other pedologists, like Nikiforoff (1949), have either accepted this view or come to a similar conclusion.

An interesting geochemical approach to the significance of plants has been made by Polynov (1948). He discusses the theoretical order of migration of widely distributed chemical elements, such as K, Na, Ca, and Mg, based upon the known physico-chemical properties of their compounds, relative to the "actual" order of migration, as indicated by the composition of river and sea water. The theoretical order varies widely from the actual order, thus:

Theoretical order of migration (After Goldschmidt, Fersman)	Fersman's corrected order of migration	Polynov's actual order of migration
K	Na	Ca
Na	K	Na
Ca	Ca	Mg
Mg	Mg	K

Polynov explains the divergency by the fact that the waters feeding rivers do not simply dissolve primary minerals, or country rock minerals, but pass through the soil, where they also receive materials from living cycles, which are in a different form. "This happens by a series of very complicated processes because the retention and absorption of elements is affected not only by organisms but also by soil colloids, the formation of which is related either directly or indirectly to the activities of organisms." Polynov proceeds to group all the commonest elements into classes based on the "order of migration," and attempts to "decipher" a series of representative serozem, chernozem, podzol, and krasnozem ecosystems and to relate them to "the laws of the migration of the elements." Over this series the role of plants varies, being rather weakly expressed in the serozem, where the vegetation is scanty and xerophytic, and much more strongly expressed in the chernozems, podzols, and especially the krasnozems. The essential character of the krasnozem he considered due to the extended growing period over which the luxurious subtropical vegetation absorbs sesquioxides, thereby retaining them for long periods in the soil.

It has been shown by a number of pedologists that atmospheric sources of salts and bases might be highly significant in soil formation. For example, Teakle (1937) has demonstrated that the sodium chloride content of rainwater in western

Australia is a function of distance from the ocean, while Crocker (1946), and others, have indicated the importance of loess accessions—very find dust settling under the influence of gravity. A particularly important paper relative to outside accessions is a recent one by Ingham (1950), for it throws considerable light upon how very small particles in the atmosphere, too small to settle by gravity, can contribute to the soil system. Ingham's data show that under his conditions significant amounts of nutrient elements are adsorbed by the organo-mineral fraction of the surface soil directly from the atmosphere. Larger amounts are brought down by rain falling through the air from a considerable height, while even larger amounts are contributed by rain which washes the leaves of vegetation. While the latter includes materials either washed out of the leaves themselves or as exudations on the surface, the main source would appear to be atmospheric adsorption. Some idea of the order of contribution (soluble material only) from this source is indicated in the following table taken from Ingham's paper.

	Rain only		Drip from Tree		Difference—Tree only
	p.p.m.	lbs./acre/ann.	p.p.m.	lbs./acre/ann.	lbs./acre/ann.
NH_4—N	0.86	9.5	2.6	28.6	19.1
NO_3-N	0.10	1.1	0.1	1.1	—
Cl	14.8	162.8	37.4	411.4	248.6
CaO	8.7	95.7	30.7	337.7	242.0
P_2O_5	0.34	3.7	2.0	22.0	18.3
K_2O	3.9	42.9	35.2	387.2*	344.3

* Corrected figure provided by G. Ingham.

Thus the long held view that the role of plants in enriching or maintaining the nutrient and base level is essentially through the cyclic contribution of plant residues needs to be amended to include vegetation-determined accessions from the atmosphere.

Despite the general interest in soil and vegetation relationships, the real significance of the importance of plants in soil formation, relative to the other factors, is in general far from clear. The same Great Soil Groups may occur under widely different types of vegetation on different continents, as podzolized soils under eucalyptus dry sclerophyll forest and under coniferous forests, in Australia and Europe, respectively (Stephens, 1950), and the chestnut soils (red brown earths) under grass savannah in the United States and under savannah woodland in Australia. The essential unity of role between such vastly different vegetation types as coniferous forest and eucalyptus sclerophyll forest can only be determined by a full knowledge of the evolution of the soils in the two regions, in relation to all the pedogenic factors. Such information is not at present available. The plant factor is important, but how important is still not clear. The functional factorial approach offers some prospect of discovering this.

Plants as Independent Variables

It seems quite clear from Dokuchaev's writing (1881, 1883, etc.) that although he envisaged that climate, parent material, relief, and time could vary independently of each other, he did not envisage vegetation as varying independently, especially independently of climate. Indeed Dokuchaev's discussions of the vegetation factor, like most such subsequent discussions, are all rather diffuse, either because of the inherent difficulties of separating the independent and dependent aspects of vegetation, or because of a disbelief in any independent plant factor. Robinson (1949) has said, for example, that vegetation cannot be regarded as an independent factor because it is itself controlled by climate.

It became necessary for Jenny in his functional factorial approach to attempt a separation of these dependent and independent aspects. The problem he faced is clearly seen if one considers a climofunction,

$$s = f(\text{climate})_{o,p,r,t.}$$

Here only climate is permitted to vary significantly, and the other factors must be held approximately constant. While parent material and time may show little variability, and sites might be selected where the topography (relief factor) is "uniform," how can the plant factor remain approximately constant if climate varies? This problem is overcome, in part, by the ingenuity of Jenny's (1941) definition of the plant factor (given previously) not as vegetation, but as the potential vegetation. What Jenny appears to mean by the plant aspects of the independent biotic factor is the incidence of disseminules of species and biotypes arriving at a particular site—as he explains it, it might be more accurately described as the *total plant disseminule* factor. The definition is

considered equally applicable in obtaining biofunctions, such as

$$s = f(\text{plants})_{p,r,el,t,o-v,\ldots}$$

and in the arrangement of biosequences (Jenny, 1946). It is this total disseminule factor to which the regional flora is said to approximate. Jenny has stated as an ecologic principle: "If in any region the plant species or plant communities are dependent variables, their distribution being a function of one or of several soil-forming factors, the biotic factor may be obtained, as a first approximation, by enumerating all plant species growing within the area."

The above view of the independent plant factor has recently been partially accepted by Major (1951) who, discarding frequency aspects, has defined it as "the summation of different kinds of viable plant disseminules reaching the area in question." Yet, if such definitions and their approximations theoretically dispense with one problem, in practice the solutions raise others. Many things other than regional flora must be considered, for the disseminule factor depends upon dispersal capacities, and in so far as these are determined by location, the patterns of distribution, that is, the ecologic or vegetation aspects as determined by other-than-inherent qualities, are concerned. This is probably partly what Rode (1946) meant by distant action of neighboring ecosystems as a cause of soil evolution, to be discussed later.

The question arises as to what degree the regional flora is a measure of the total disseminule factor? That is, what reliance can be placed in the approximation suggested by Jenny? We are immediately faced with the problem of deciding how large is a floral region! Where does one flora end and another commence?

In the determination of the size of a floral area Jenny has given us very little to go upon. Indeed, he has rather confused both pedologists and ecologists by employing, without adequate explanation, two very different interpretations. On the one hand, in the accumulation of climo-, topo-, litho-, and chrono-functions and sequences he has used the independent plant factor essentially as he had defined it, as the total disseminule factor approximated by the regional flora. On the other hand, in considering biofunctions and sequences he decided to make the best of both worlds, and has used the flora of particular plant communities, even when considering closely adjacent locations. That is, adjacent areas are treated as having distinctive independent plant factors, even though there is considerable similarity in their total disseminule factors. Thus in considering the forest-prairie transition zone in the midwestern United States, Jenny has made the assumption of "two distinctly different and independent biotic factors." This dual use of vegetation as either dependent or independent is disconcerting, even if convenient. The apparent inconsistency has not been discussed by Major (1951), but it requires further discussion, and this will be undertaken at a later stage of this article.

In an extremely broad way one can look at the plants of the world as comprising one huge flora. From this viewpoint the actual floral patterns have been segregated by the selective factors of climate and soils (and soil parent materials), by competition, by organic evolution, by chance phenomena (including isolation), and probably by other factors. The basic segregation depended primarily upon climate, and is probably very ancient, although there may have been considerable fluctuations and changes of relative area. From this viewpoint the general flora would be the independent variable, the regional flora, dependent. For application in Jenny's quantitative approach, however, the regional flora must be independent. This anomalous situation is readily resolved. The regional flora can be considered independent because the basic segregation into regional floras preceded soil-formation time zero.

Plant geographers have made numerous attempts to delimit floral regions. One such is inherent in the *phytogeographic element* of Braun-Blanquet (1932), and the matter is broadly discussed by Wulff (1943). However, while isofloral maps and common observation may indicate that certain areas possess a large number of distinctive species and forms, in general absolutely precise and distinctive floral regions can never be isolated. This is because of differences both in ranges of tolerance of biotypes and in their histories within any area, relative to environmental variability. In practice the plant geographers often classify species in a particular region as *intraneous* or *extraneous*, according to whether the occurrence is located well within or toward the periphery of their general area of occurrence (Cain, 1944). Such arbitrary divisions serve to emphasize some practical limitations in trying to separate floras. Most distinctive floras are to be found where a large

number of species have been isolated from surrounding floral areas by major barriers to plant migration, such as the ocean or a profoundly different soil or climatic environment, and have experienced a common environmental history. On the broad scale, such areas might be represented by continents, or large portions of continents, and at the other extreme by the plant community. All degrees of isolation, that is, all degrees of effectiveness of barriers, occur, and absolute isolation is rarely if ever achieved for long.

In nature, then, absolutely distinct floral regions do not exist. Yet, if the plant aspect of the independent biotic factor, the total disseminule factor, is approximated by the regional flora, some decision must be made about extent of region before floral lists can be compiled. Only on the basis of such a decision can the closeness of the approximation and its real merit be assessed. But first it becomes necessary to make a more complete analysis of the total disseminule factor.

To initiate an analysis of the disseminule factor let us put aside the problems of determining soil-formation time zero by taking a simple case. We shall ask what determines the total disseminule factor at any entirely new site at the initiation of soil formation. Essentially it is the location of the site relative to (1) the origin (releasers) of disseminules, (2) the agents of dispersal, and (3) barriers to dispersal. The disseminule factor, at soil-formation time zero, is essentially determined, therefore, by the past history of the floral patterns of the region—evolution, migration, environmental selection—a history which is very closely bound up in all its aspects with the evolution of the general landscape, for environmental evolution may impose a directional effect upon organic evolution (Timoféeff-Ressovsky, 1940; Darlington, 1940; etc.).

Disseminules arriving at the theoretical new site at initiation of soil formation do not have equal potentialities for occupancy, and hence even potentially equal roles in pedogenesis. Omitting, for the present, the frequency aspects and competition effects, the disseminule factor, as used by Jenny and Major, can be divided into (1) biotypes whose ranges of tolerance lie wholly outside those of the site, and (2) biotypes whose tolerance ranges overlap the particular environment. Thus the total disseminule factor includes an ecosystem-excluded flora, and an ecosystem-suited flora. In the ecosystem-suited group we can include those biotypes which might not be initially capable of establishment, but which as a result of new niches appearing during vegetation development, are "potential" tenants. Uniformity of the soil-forming factors is of course implied. This ecosystem-suited flora is not synonymous with the actual flora to be found at any particular moment, although it might closely approach it. The actual flora, of which the plant community is an organized expression, has some elements excluded as a result of competition, and others as a result of the time state of the ecosystem. The general breakdown can be summarized thus:

Total Disseminule Factor ↗ *Ecosystem-suited Flora*
- (i) *Actual flora* — *Pedogenetically effective*
- (ii) *Lost developmental element* — *Pedogenetically effective*
- (iii) *Potential developmental element*
- (iv) *Competition-excluded element*

Total Disseminule Factor ↘ *Ecosystem-excluded Flora*

The ecosystem-excluded flora, as defined above, has absolutely no role to play in soil formation, and cannot be considered therefore as playing any part as an independent plant variable. Variations in the ecosystem-excluded group can be as broad or narrow as one likes without resulting in any changes in the soil system, apart from the additions of the disseminules themselves. Jenny's definition therefore requires more precision. It is not the total disseminule factor, but the ecosystem-suited portion of it which is significant in soil formation. The relative size of the suited and excluded groups will be determined largely by the specificity of the new environment relative to surrounding ecosystems. If we consider, as a theoretical case, soil development proceeding in our example under uniform conditions for an infinite time, then an accurate measure of the biotypes concerned in the ecosystem-suited group would be given by those in the vegetation at time infinity, t_∞, plus those excluded or lost during vegetational development. At any time t_p, the closeness with which the actual flora gives a measure of the ecosystem-suited disseminule factor will depend essentially therefore upon the biotypes excluded by competition and the time-state of the system, relative to those currently included in the plant community.

Not all of the ecosystem-suited disseminule factor has pedogenic significance. In the simple case

we have been considering the flora drawn from the actual vegetation might approach the independent plant factor very closely. For, if we do not attempt to prophesy the tend of future soil evolution and concern ourselves only with current properties, the "potential" developmental disseminule element must be neglected. It has played no part in soil formation to date. Similarly, the suited disseminules which have been completely excluded for reasons of competition can be neglected, for they have played no part in soil development. Thus at t_p, in our case, the flora of the actual vegetation differs from the suited disseminule factor effective in soil formation only in the exclusion of those biotypes lost during its development (succession) from time zero. For convenience of future discussion I propose to call the portion of the suited factor measured by the actual flora plus this lost developmental flora the *effective disseminule factor*.

We have considered a very simple case. Let us proceed to a more complex one, where instead of having, as in the last example, a uniform parent material, of regular relief, under specific climatic conditions, being suddenly presented to a specific array of plant disseminules, several distinctive (contrasting) parent materials are so presented. After a given time, t_p, we can then explore the soils and vegetation developed on the different parent materials. As in our foregoing example, on each specific parent material the floristic composition of the vegetation at t_p will be a close approximation of the significant plant factor during pedogenesis on that particular site. The degree of approximation on the different parent materials will depend upon the relative losses (exclusions) that have occurred during vegetation development. The state of each such ecosystem at any time is defined by a specific combination of factors. If we now widen our analyses to investigate differences between soils developed on each of the several parent materials, that is, a lithosequence,

$$s = f(p)_{cl,o,r,t,\ldots}$$

we are faced with the problem of determining what is the significant plant aspect of the biotic factor, o. In this case we have done nothing more nor less than widen the ecosystem. The effective plant factor over the system as a whole is the summation of the effective disseminule factors of the smaller included systems as determined by the distinctive parent materials. Thus the best approximation of the significant plant factor for the wider system under analysis is to be had in the summation of the individual floras within it. This approximation will differ, as before, from the true factor only by those biotypes lost during vegetation development from t_o to t_p. Within the wider system, the vegetation at any specific sites at any specific time is dependent, in our example, only upon variations in parent material.

The pedogenetic significance of the biotypes lost during vegetation development will vary. Some of those which were prominent in the early stages may leave no trace upon soil properties at t_p. Thus the effective pedogenic plant factor may approach even closer to the ecosystem flora.

The foregoing two simple cases have been discussed rather fully. They may serve as a basis to discuss the limitations and the possibilities of distinguishing in the field an independent pedogenic plant factor. Both cases may be approximated in nature. New parent materials are continually being presented to the influence of the soil-forming factors. Soil development might also proceed with the colonization of newly presented parent materials which are both diverse and occupy a considerable area. Such an example is soil formation subsequent to the Mid-Recent Xerothermic Period in Australia (Crocker, 1946; Crocker and Wood, 1947). In general, however, the situation in nature is much more complex. Continuity of ecosystem-determining factors is rarely maintained for long, and all degrees of variability between ecosystems occur. Furthermore, a change of system often presents a new parent material which, at the outset, is not devoid of vegetation. Indeed it seems to the reviewer that the complexity of ecosystems and their evolution prevent the general possibility of assessing the pedogenically significant plant factor. However, this does not necessarily preclude special cases—cases which might be used to accumulate functions and sequences. The generalization can be drawn from our simple theoretical examples that the *effective disseminule factor* is the independent plant factor in pedogenesis. It is independent in the sense that the suitability of the actual ecosystem and its parts is determined quite independently of the ranges of tolerance of the biotypes, and bears no relationship to their relative frequencies at the initiation of soil formation. As Cain (1944) has said, "dissemination is essentially a chance phenomenon in the sense that there is no co-operation between disapores and agents to assure the move-

ment of a diaspore to a place that is suitable for its germination and establishment." What, then, is the best approximation that can be made of the effective disseminule factor in nature? To answer this we must look into certain assumptions that have to be made, for without making some assumptions little can be achieved.

As far as the "kind" aspects of the independent plant factors are concerned, we saw from the simple examples discussed that the biotypic composition of the present vegetation of the system might be a close approximation. The closeness of this approximation depends essentially upon the significance in soil and vegetation development of the biotypes lost since time zero. Unless the time from initiation of the ecosystem is very long, many purely developmental biotypes are still likely to persist in odd niches. In taking the composition of the present vegetation as representative of the biotypic aspects of the independent plant factor, the assumption is always made, therefore, that the biotypes lost since time zero are insignificant. The reasonableness of this assumption is one thing which determines the value of interpretations made and relationships established. It may frequently be possible to get some measure of the biotypes lost by means of plant succession studies in the area.

In some situations the time-state of the ecosystem might be such that relics from an earlier system still persist, even though under the present conditions they are unable to re-establish themselves—that is, they are not part of the current effective disseminule factor. For example, to take an extreme case, a long-lived tree like the California redwood *Sequoia sempervirens* might persist, once established, for a considerable period after it ceases to be part of the effective disseminule factor. Its ultimate extinction might be inevitable, but until it dies it remains as part of the initial state of the system that has persisted. Thus not only might a floral list be an incomplete representation of the current effective disseminule factor, but it may also include biotypes which do not rightly deserve inclusion.

In the accumulation of lithofunctions, chronofunctions, topofunctions, and climofunctions, two assumptions concerning the independent plant factor are necessarily made. Firstly, it must be assumed that over the ecosystem being considered, as a whole, variation in the effective disseminule factor has been pedologically insignificant. Secondly, it is assumed that variation of the effective disseminule factor with time, from t_o to t_p, has been likewise insignificant. Any significant change produces a new ecosystem. Variations in the independent plant factor, that is, the effective disseminule factor, with time, are principally caused by (1) phylogenetic evolution, (2) additions resulting from the ecosystem coming within the dispersal capacities of new migrants, (3) chance dispersals due to some extraordinary cause, and extraordinary influences of neighboring or distant ecosystems, and (4) minor fluctuations of the long-term climate which have autecological or physiological significance.

Phylogenetic evolution is so slow that it can probably be disregarded over the period that the conditioning factors of the system have been otherwise stable. Under certain circumstances migration effects may be highly significant. Chance dispersals and minor climatic fluctuations may be of considerable importance in effecting both the kind and frequency of disseminules during early stages of those systems with a large number of vacant niches at the time of their initiation (e.g., bare areas). Indeed, the pedological significance of variations within the disseminule factor might change considerably with the time-state of any particular ecosystem.

Frequency aspects were recognized as being included in the independent plant factor by Jenny, without any discussion of their significance. Major (1951) has discarded frequency considerations in his vegetation system approach. The frequency of disseminules arriving from outside the system could probably be reasonably approximated by direct measurement. Considerable short term variability is certain. Relative quantities of biotype disseminules are only important in soil formation if they influence floristic composition. Even so, only such frequencies as have been significant in determining soil properties at t_p need be considered. At all events the assumption that the independent plant factor has not varied significantly from t_o to t_p, or over the ecosystem as a whole, includes the assumption that variations in frequencies of effective disseminules has been pedologically negligible. The significance of variations in frequency might diminish with time, and, as with biotype variability, can be expected to be more significant at the initiation of soil formation on bare areas. In the absence of data for disseminule frequencies and time variabilities, there is probably no alternative in practice but to

omit it from consideration; nevertheless one should always be alert for its consequences.

The important thing is that the assumption that the independent plant factor has been "constant" in any particular ecosystem requires careful consideration before acceptance. The reliability of the assumption will be closely connected with the regional extent of the ecosystem relative to surrounding systems and their time-states. For example, climatic change, other things being equal, has a sifting effect upon the botanical composition of plant communities (Crocker and Wood, 1947) so that the most functions and sequences can usually only be achieved over a narrow range of climatic variability. Absolute uniformity of the disseminule factor is never achieved, but pedogenetically significant uniformity might be approximated. It is not required that there be no variability, but only that variability, either with time, or within the system at any particular time, has had no importance in soil formation.

Not only are the disseminules arriving at a particular site not all effective in soil formation, but those which are effective are not equally so. There are differences in degree of effectiveness. In a list of the total flora, biotypes which to all intents and purposes are insignificant are given equal rank with those which are extremely important. Unfortunately the pedological significance of different biotypes is difficult to arrive at. There have been no theoretical reasons advanced that dominant biotypes, in the ecologist's sense, are in themselves always more directly important in soil formation that subdominant types. Indeed, Braun-Blanquet (1932), in recognizing that species vary greatly in their ability to influence the habitat (of which the soil is part), has stressed that this ability ("constructiveness") is not necessarily associated with dominance. Nor is the situation straightforward, for biotypes which are in themselves unimportant might be important in producing suitable habitats for highly significant types. An excellent example of the variability of pedogenetic effectiveness of biotypes is the local podsolization caused by Kauri pine (*Agathis australis*) in New Zealand—an extreme case where every tree has its own profile. If the pedogenetic significance of more groups were known, a simpler but equally good approximation of the independent plant factor might be obtained by consideration of only a few biotypes. The kind of information required to do this could best be obtained by autecological studies and experimental studies in pedogenesis, including the soil-forming processes. Another, and more subjective, approach is to use something like Braun-Blanquet's "constructiveness" estimates, which although based upon observation and inference may not be unreasonable. It will probably be usual, in practice, to consider the whole community or communities as an index, unless for some good reason, as with the Kauri pine, something more restricted can or should be used. In the final analysis it is the succession of plants from t_o to t_p on the individual profile sampling site which has been the effective plant factor on that site. If over some wider area embracing a plant community there are no apparent soil differences, it is reasonable to consider the flora of the community as a whole as an approximation of the significant plant factor.

Although the foregoing analysis of the independent plant factor is incomplete, it is adequate to formulate an answer to our original queries as to the reliability of Jenny's regional flora approximation, the extent of the region to be considered, and the best approximation possible. The extent of the region can obviously be as large or as small as required, consistent with the reasonableness of the assumption that the ecosystem *as a whole* has had the same pedogenetically significant disseminule factor. That is, any variability has not been significant in soil formation. The extent will depend in investigational work, within the variability of the disseminule factor allowable, essentially upon the limits of the ecosystem being studied and *should be restricted to these limits*. The best measure of the biotype composition aspects of the independent plant factor is a floristic list of the pedogenetically significant biotypes within the system, together with those significant ones which successional studies indicate have been lost during development. Providing one limits the region as above, and such limits are more or less inherent in Jenny's ecologic principle, the "first approximation" is a good one. Indeed our different and somewhat contrary reasoning has led us to a more precise but very similar approximation. As far as the "kind" aspects are concerned, Jenny's approximation largely errs on the conservative side, by being too inclusive and by giving equal status to pedogenetically significant and insignificant biotypes, thus magnifying differences. For these reasons, and for the reason that important developmental biotypes might have been omitted, differences between and variations within independent plant factors

might be either exaggerated or overlooked. In general they are likely to be exaggerated, which is hardly a bad fault.

Florosequences and the Significance of the Prairie-Forest Transition

The study of soil formation as a function of some property of vegetation, as

$$s = f(\text{plants})_{cl,p,r,t\ldots}$$

is especially difficult in nature. In recognizing the difficulty Jenny points out that "only that aspect of vegetation is considered a soil-forming factor that cannot be correlated with climate, parent material, topography, and time."

In some degree, of course, the flora is always dependent upon climate. Indeed, the independent plant factor is unique in that while theoretically climate, parent material, and relief may be found in almost any combination, the basic climatic patterns of the earth do impose very definite restrictions upon the composition of the total disseminule factor in any region. This has led to a great deal of confusion among pedologists and ecologists (Stephens, 1947; Robinson, 1949; etc.) in interpreting Jenny's biotic factor. The confusion need not have arisen had Jenny indicated that such basic climatic selections occurred, as pointed out earlier, prior to the initiation of any soil system capable of being investigated by his technique.

Florosequences or functions are only reliable where similar time-states associated with pedogenetically identical climates, parent materials, and topography have distinct effective disseminule factors. This can best be achieved under controlled conditions, or where ecologic barriers have maintained some sort of isolation. The fact that the effective disseminule factor is so continually varying in nature offers other opportunities of analysis of a more refined type providing a reliable measure of the variation can be made. While very few florofunctions can be hoped for, sequences which illustrate some general trends are possible. Nevertheless very few such sequences have been studied, and even where they have, doubts about the assumptions of uniformity make them suspect. Perhaps the most interesting attempts have been concerned with the forest-prairie transition zone in the United States.

In considering the prairie-forest transition, Jenny has concluded that adjacent prairie and forest can be treated as distinct soil-forming factors. This use of the biotic factor varies from that he employed when seeking chronofunctions, lithofunctions, climofunctions, etc. The dual treatment is worthy of further consideration, for on analysis it appears not only less inconsistent than on the surface, but it provides a very good example of some of the limitations inherent in the method.

It is not clear upon what Jenny has based his assumption of "two distinctly different and independent biotic factors," as his discussion is sketchy, but it appears to rest chiefly upon the role of higher organisms, buffalo, man (fire), etc., in maintaining the boundary. If this is so, then the subsequent comparisons Jenny has given between prairie and timber soils as examples of the operation of the independent plant factor are not valid. Here the distribution of plants is dependent, being dependent upon higher organisms, and therefore the soil differences are, other things being equal, functions of higher organisms. Perhaps Gleason's (1923) theory of active forest migration in response to climatic change is a better reason for recognizing two distinct biotic factors. This migration now appears to be widely accepted. It is also believed to be a feature of the forest-steppe transition in the Caucasus (Zakharov, 1935). In such cases the situation is complex for the vegetation boundaries are then largely a function of the time-state of the system. At all events, the prairie-forest transition in the United States is particularly interesting because the differences in life forms of the two formations are so striking and soil changes parallel the vegetation transition. Yet there is frequently no apparent correlated change in soil parent material, topography may vary little, and the climatic gradients appear negligible. Another interesting feature which has stimulated much ecologic inquiry is the persistence in the transition zone of grass areas within the hardwood forest. The soil changes associated with the transition have recently been discussed by Smith, Allaway, and Riecken (1950) and are summarized in a modification of their schematic diagram in Fig. 1.

It is in considering soil differences across this transition that Jenny has invoked the operation of two distinct independent plant factors. His interpretation considers the differences as functions of the two plant factors and implies therefore insignificant variability of the other soil-forming factors. Is this interpretation justified? The question can be approached by examining the situation exhibited in the above diagram. It is apparent that a

distribution of the type shown would not be possible, as a regional feature, if parent material, climate, relief, time, and the biotic factor (including the frequency and types of plant disseminules), had been "constant." This means either that two independent plant factors have operated, or that there has been variability of selective significance in either the parent material, the climate, the relief, the time factor, the non-plant aspects of the biotic factor, or more than one of these.

Considering the prairie-forest transition zone as a whole, the situation is very complex and almost all the above situations occur. In many places the boundary conditions are unquestionably determined by relief (Gleason, 1923; Smith, Allaway, and Riecken, 1950). As the general transition is along a climatic gradient, climatic effects cannot be entirely ignored. The ecological evidence for encroachment of forest upon the grassland, backed up by post-glacial climatic history, indicates that the time-state of different parts of the section of the system associated with forest varies. This difference in time-state might be essentially the reason for the variation shown between *c* and *d* in Fig. 1. Encroachment also means that across the advancing boundary the effective disseminule factor has been changing as portions of the area have come within range of the dispersal capacity of the migrants. It is possible because of the latter to select localities where the soil differences are essentially a reflection of the influence, at a specific time-state, of a change in the independent plant factor. The plant factor at *a* in Fig. 1 may differ from that at *d*, for example, in this way. Nor can parent material differences, as a reason for soil-vegetation variation, be omitted, despite the considered homogeneity of the underlying loess. Differences in the state of the system at t_o (i.e., when significant forest disseminules first arrived) are especially important in those cases where large grassland inclusions have persisted within the forest. It is not the state of the soil itself that is important at t_o but the state of the ecosystem, that is, vegetation must also be included because it influences selectivity of the habitat. The only conclusion that can be reached about the relic grassland areas, for example, is that differences of selective significance, even if subtle, occurred at and have been maintained since t_o—that is, "parent material" differed.

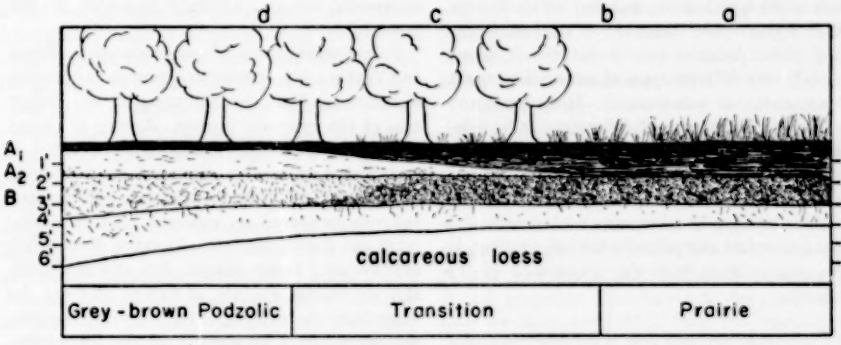

FIG. 1. SOIL-VEGETATION RELATIONSHIPS IN THE PRAIRIE-FOREST TRANSITION ZONE

The presentation is idealized, but shows the main soil changes associated with the transition. The decrease in organic matter, the development of a podzolic A horizon, and increased structural development of the B horizon are the main features differentiating the Forest Soils from the Prairie Soils.

The differences in soil properties cited by Jenny in his comparisons of the prairie and forest soils show that both organic matter and total nitrogen are more abundant, and have different types of distribution in the prairie soil. On the other hand, translocation of mineral material has been greatest under the forest. He concluded that "under equal climatic circumstances a deciduous forest cover stimulates leaching and accelerates soil development." Whether the data cited by Jenny pertain to sites which rigorously meet the distinct flora requirements is questionable. However, by comparing the properties of adjacent Prairie soils and Grey Brown Podzolic soils on comparable slopes,

and from the same parent materials, Smith, Allaway, and Riecken (1950) have demonstrated essentially the same points. The effectiveness of their site selection might also be questioned to some degree. Nevertheless, these are, perhaps, the most significant biosequences that have been described, and the assumptions made can probably be considered quite reasonable for the type of generalization concluded. More detailed conclusions would need to be accepted with caution, their value depending upon the degree to which the assumptions of uniformity or variability were valid.

The foregoing and brief discussion of the forest-prairie transition zone illustrates a number of important points relating to the independent variable approach of Jenny. It indicates how complex situations might be in nature, and that within the one limited region where variability of climate, topography, parent material, and the disseminule factors are small, very different types of assumptions might be reasonable or unreasonable. It demonstrates the fact that subtle or small differences in the independent factors may have profound pedologic significance. As uniformity of the independent plant factor is rarely if ever absolute over any area, assumptions of uniformity (pedogenetically) require therefore exceptionally critical scrutiny. It demonstrates once more the importance of the ecosystem.

SOIL EVOLUTION AND POLYGENESIS

The environmental soil-forming factors are not constant and may change with time. From this, concepts of soil evolution have emerged. Current soils merely represent stages in a continuous process of their development. They have been different in the past and may further change in the future.

Autogenic Soil Evolution

The idea of autogenic evolution of soils was apparently first introduced by Kossovich. He developed the hypothesis that self-development of the soil could occur independently of the environment (Gerassimov, 1935; Nikiforoff, 1942; Yarkov, 1949), and that the developmental processes could proceed either without, or despite, changes in environment.

One aspect of autogenic evolution has recently been brought forward as a new concept under the name of "pedogenic inertia" by Bryan and Teakle (1949). In their studies of basaltic soils in eastern Australia they discovered that over uniform parent material and similar degrees of slope, deep red soils occur side by side with shallow black soils. The former have kaolinitic clays and sesquioxides in the colloidal fraction, while the latter have a dissimilar sesquioxide distribution, and the clay belongs to the montmorillonite-beidellite group. Their explanation is that the red soils were formed under an earlier, and different, climatic regime, and that the soil-forming processes initiated then have since persisted. The black soils are believed to be a product of the present climate, having been formed from the parent basalt where this had been exposed by erosion of the earlier red soils. Pedogenic inertia is defined as "the concept that a process in soil formation, once established, may continue in spite of changes in the environment to conditions which apparently are not normally favorable to this process."

Unfortunately the evidence produced by Bryan and Teakle is too sketchy to permit the evaluation of their hypothesis. No indication of the absolute ages of the two soils is given, identity of parent material is stated but not established, nor is it explained how erosion managed to completely strip the old soil from certain areas, while a deep soil was preserved on adjacent sites of similar relief and parent material. Assuming their differential erosion theory correct, it is still not justifiable to assume identity of parent material, for while from the stripping of the old soil to the present there would be identity of the soil-forming factors of time, climate, and relief, and, in Jenny's sense, vegetation, in one case parent material would be bare basaltic rock, and in the other an old and fairly deep soil representative, practically, of an end point of weathering (lateritic). It is more than likely that there have been subtle changes in this old soil since formation of the black soils was initiated; indeed there may have been no "inertia" at all.

Evolutionary Cycles and Contradictory Processes

Other aspects of autogenic evolution include those developments which have been built on the soil-evolutionary theories of the Russian agronomist-pedologist, Williams. In part these were a reaction against the sterility into which the zonal concepts had drifted, in part they were an attempt to orient theoretical pedology in the direction of dialectic materialism. Williams emphasized that, under the influence of biologic and climatic factors, soil formation takes place essentially in time, and

that it is absolute time which is of decisive importance. He introduced the concept of a single universal soil-forming process, of which different phases were represented by current soil varieties. Existing soils were merely the expressions of different stages of this process. The main features of these ideas are outlined in the part of *Pochvovedenie* (1935) dedicated as a memorial to Williams' contributions to pedology. Williams claimed that the space stages gradually replace one another in time, and that these reappear ultimately in evolutionary cycles. These cycles are not repetitions, however, because the universal process always meets new variants of relief, parent material, etc., and might retain certain qualities from earlier cycles (Bush, 1935). The cycles are never entirely closed because of the residual variation (Rode, 1946), and therefore the net result is an evolutionary spiral. The cycles themselves begin with the initiation of the accession of organic matter and attendant decomposition to humus and other products. This process reaches a maximum, parallel with maximum fertility, and thence declines to nothing. The final stage of soil-formation is "the appearance of complete infertility in the soil, the dispersal of the elements of nutrition throughout the mass and impermeability by water . . . the soil has returned to the condition of the original rock" (Bush, 1935).

One of the main arguments presented by Williams for soil evolution was apparently the fact that, since the most southerly advance of the glaciers in Russia during the Pleistocene, the retreats of the ice have successively exposed new areas to soil formation. The absolute ages of the soils, therefore, are greatest in the south, towards the deserts, and become progressively less as one proceeds north. Every zone farther south has passed through all the stages of soil formation observed north of it, and every northern zone will inevitably pass through all the stages now observable farther south (Bush, 1935; Spirhanzl, 1950). It is to be noted that time is on a geological scale in Williams' concepts, and that the single universal soil-forming process he envisages is essentially related to cycles of fertility, themselves a product of the accession, breakdown, and translocation of organic remains. All significant soil properties are believed bound up with this organic cycle. The main achievement of Williams' pedogenic concepts, as apart from his agronomic contributions, appear to be that they emphasize the importance of organic matter.

Acceptance by Russian pedologists of a single soil-forming process and the space-time relationships inherent in it has not been general, however, and has in fact been severely criticized in places. The chief arguments have been that different vegetation types produce qualitatively different humus, so that under different climates and geographical environments the soils vary qualitatively from the very beginning of their formation (Remezov, 1932; Gerassimov, 1935); a definite series of soils representing the stages and phases of the universal process of development is therefore peculiar to each of the major climatic zones (Gerassimov, 1935). The emphasis on fertility with its practical implications, the historical evolutionary perspective, and the manner of reasoning connecting the two, have apparently reestablished Russian pedology on the principles of dialectic materialism (Krzhizhanovski, 1935; Bush, 1935; Yarkov, 1949; Gerassimov, Lobovoi, and Fridland, 1950; Spirhanzl, 1950; *Pochvovedenie*, 1950).

Williams always stressed that the accession of organic substances in nature was accompanied by decomposition, and that the single soil-forming process developed to a maximum and then dwindled to nothing. This idea has been expanded by some of his followers, even though they did not necessarily accept the notion of a single universal process, into the concept of *contradictory processes* in soil evolution. The theory is discussed fairly fully by Remezov (1932). Soils are regarded as undergoing continual changes, the intensity and direction of which are conditioned by contradictory processes occurring in them. Several processes may occur in each soil simultaneously, but one is always predominant, the others being either at some submaximal stage from initiation, or in a stage of decline. The dominant process determines the character of the soil. The development of each process inevitably leads to its extinction and to the intensification of the opposite process which is to supersede it (autogenic evolution). Qualitative changes associated with these maximal expressions of processes result in a transition of one soil variety into another and the initiation of new contradictory processes at each stage. During their evolution soils pass through a number of consecutive stages from more primitive to more complex forms which are genetically linked. "Each stage is a consequence of the previous stage and the beginning of the successive one. On each of its stages soils carry the diminishing traces of their past and the increasing beginning of their future. Thus, the contradictory

processes occurring in soils condition their motion, their transition from one quality into another." The soil-forming processes themselves, all of which in their proper sequence are contradictory to each other, appear to be identical, as described, with the processes of great-soil-group formation, for, to quote from Remezov, "in the southern part of the chernozem belt the humic processes meet with contradictory solonchakous, solonetzous, and solodization processes; in the northern part, the contradictory processes are those of degradation and podzolization."

Polygenetic Soils

Williams' pedogenic concepts have directed attention to the fact that soils may be polygenetic, and are not necessarily to be regarded as results of the currently operating factors of climate, relief, parent materials, and organisms. The Russian school in developing the concept of continuous change has discarded the long-held ideas of soil stability and equilibrium which are incompatible with it. Soil zonality concepts have also been abandoned, as well they might have been in their then current form, and the study of soils in historical sequences, the evolutionary position of the space groups, have become important. Williams' ideas have finally been widely accepted in Russia, and in the opinion of Remezov (1932), "his achievement is enormous in that his work constituted the turning point in the history of pedology." Recent work, such as that of Akimtsev (1945), Rode (1946), Bolyshev (1947), and Klopotovsky (1949), evidences that considerable importance is attached to historical changes amongst the soil formers.

The significance of the historical perspective was realized somewhat later in the west, and is developing along different lines. A notable discussion on the theoretical aspects is that of Bryan (1946), dealing with "the geological approach to soils," described as "from below and from the past." He has deplored the tendency among pedologists "to regard those soils now visible at the surface as due to the action of processes at present in operation and controlled by contemporary climatic conditions." This viewpoint, he claims, if not explicitly stated, "is implied in most of the work dealing with the origin of soils."

Most studies in which a polygenetic history has been established for soils have been concerned with past changes of climate. Both long periods of climatic stability and periods of variability are indicated in the geologic record. It is highly significant when considering soil genesis that the climate of the immediate past, the Quarternary, has been especially variable (Bryan, 1946). Prescott's (1931) recognition that the extensive "duricrust" of Australia was in large part made up of remnants of an old lateritic crust reflecting a change in climate, has proved an important introduction to a number of studies, characterized by their historical perspective, on the development of Australian soils. Bryan (1939) has described the "Red Earth Residuals," a suite of soils from Southeastern Queensland which both geological and pedological evidence suggest are relic of a different climatic environment. Whitehouse (1940), in geological investigations of the Queensland Tertiary, has also produced pertinent evidence of climatic change in the morphology of the soils of an extensive region. Likewise South-Australian studies (Crocker, 1941, 1946; Crocker and Wood, 1947) have shown that a wide range of regional soil groups possesses some morphologic characters which have been inherited from an earlier and different climatic regime. Nor is climatic variation the only type of change in the independent factors that is possible during soil development, for all of the factors can vary. Changes in parent material are quite frequent. These are well illustrated by the large accessions of calcareous loess which seem to have been a significant pedogenetic factor, especially in the mallee soils, over large parts of Southern Australia (Crocker, 1946). A notable and ingenious contribution indicating significant variability of the pedogenic factors is Butler's (1950) discussion of the soils of the extensive Riverine Plain of Southeastern Australia. Butler shows that the sedimentation responsible for the basic distribution of parent material in the region is related to a prior stream system, and that the present soil pattern can only be satisfactorily understood in terms of subsequent differential changes in the leaching factor imposed upon an earlier soil pattern.

The soils literature of Australia, illustrative of polygenesis in soils, and only partially reviewed above, is by no means unique. In addition to the Russian work cited, investigations in the United States, Europe, and Africa provide examples of the operation of multiple combinations of the independent factors in soil formation. Highly significant contributions include some of the suggestions of Joffe (1936), Bryan and Albritton (1943), Cline (1949), Hunt and Sokoloff (1949), Simonson (1949),

and Thorp, Johnson, and Reed (1951). It is unnecessary to review these publications in any detail. For purposes required here, the significant thing is this: a growing body of information indicates that a lage number of soils, indeed most soils, are the result of more than one combination of soil-forming factors, that is, they are polygenetic.

Further Views on Soil Development

An interesting approach to soil evolution is taken by Rode (1946), who, like Tansley, points out that in nature a soil is essentially not capable of isolation from the wider ecosystem (he uses "biogeocenose," after Sukachev). Rode, in conformity with his Russian colleagues, regards soil formation as cyclic. As some of the cycles are not closed, they result in permanent successional changes in the soil. The direction and character of such evolution is determined by eight soil-forming factors, and Rode adds to the five well-recognized factors gravitation, water (surface, soil, and underground), and agriculture.

Rode considers that the "transformation" and "interconversion" processes, those which give the evolutionary character to soils, operate continuously. They never entirely cease, although they might be reduced to a slow rate. They are caused by (1) variation of external factors, (2) distant action of neighboring ecosystems, (3) self-development of the particular ecosystem, and (4) phylogenetic evolution of organisms. In the latter three cases, "the most important introductory impulse of evolution . . . appears to be the life processes of living organisms, especially of plants." It is not clear to the reviewer whether "self-development" means autogenic evolution, or changes in the state of a particular system with time, but, in any case, soil formation under these circumstances "always appears as known irreversible processes."

The decomposition of primary minerals into their secondary derivatives and elemental oxides in soils is also generally regarded as irreversible, or but partly reversible, and accordingly also imparts changes in composition leading eventually to evolution.

Nikiforoff (1949), a stimulating contemporary writer on the genetic and dynamic aspects of the soil, and an interpreter of Russian viewpoints, has taken sharp issue with the concept of a continuous progressive process of primary mineral decomposition, considering it entirely untenable. Nikiforoff points out that any such continuous process of decomposition of unstable minerals must inevitably result in their depletion, leaving a static infertile mantle of residual resistant minerals, but that this is not the type of equilibrium reached in soils. "If resynthesis of the broken-down primary minerals is impossible under conditions in which these minerals undergo decomposition, and if the leached compounds do not return to the material from which they were removed, then one may infer that the steady state of the material which is affected by these processes can be maintained only by the steady removal of the worn-out epidermis of the crust of weathering, and a simultaneous sinking of the horizon in which the decomposition and leaching are taking place." In this manner a steady replenishment of primary minerals is visualized, and for degrading surfaces "the A horizon . . . develops from the material that previously passed through the stage of the B horizon rather than directly from the C material." Every degrading surface has its accompanying aggrading surface where old soils are periodically being buried to varying degrees by new deposits. In such soils "the A horizon . . . develops from the fresh assorted sediments that settle on its surface rather than from the material that previously passed through the stage of the B horizon, whereas the B horizon follows the A and develops from the sediments that passed through the stage of the A."

Nikiforoff stresses that the differences in soil formation on aggrading and degrading surfaces (he calls them cumulative and non-cumulative) are important, and believes that they are frequently given insufficient attention, despite the long period they have been recognized. This is, if anything, an understatement, and many implications of degrading-aggrading surfaces have been very effectively discussed by Bryan (1946). On degrading surfaces, fresh material practically unaffected by previous pedogenic processes is continuously coming under the influences of the soil-formers as the old solum is lost. Under aggrading conditions, the previous surface is periodically being buried. This burial may range all the way from a gradual upbuilding where sedimentation is slow, but relatively regular, to a drastic imposition of new material which might place the old profile beyond the influence of future soil formation. Nikiforoff believes that with both these extremes the specific characters of the buried soils are obliterated, any properties which are preserved being inadequate to indicate the course of evolution. Indeed, he reaches the conclusion that

there is little if any possibility for preservation in soils themselves of any feature which might have been acquired by them during the past stages of their evolution. This rather sweeping conclusion has its foundation in the acceptance of the viewpoint of the Williams school that organisms, cycles, and contradictory processes are of over-riding significance in soil formation.

The ideas of cyclic processes in soils developed by Williams and his colleagues were assumed to depend basically upon the interaction between organisms and rhythmic fluctuations in solar radiation, on a time scale varying from diurnal to geologic (Bush, 1935; Rode, 1946; Yarkov, 1949). These interactions were considered cyclic because solar energy fluctuations were cyclic. Nikiforoff has interpreted the solar radiation variations as a succession of waves, reserving cyclic processes for "the chain transformation of the matter and energy in the course of which the matter and the energy periodically reappear in the same form." He decides that all cyclic soil-forming processes are maintained by organisms, and that this is "the principle determining all pedogenic processes." Having thus defined only those soil properties related to organic cycles as pedogenic, it is easy to conclude that burial, or change in the cycles, will lead to the slow, though not as he maintains the "self-", destruction of existing organic characters. Hence characters acquired in earlier stages of evolution are not preserved. Such deductions, however, are not absolutely sound, being based on incomplete assumptions, and being at variance apparently with many of the facts in nature.

The assumption that organic cycles determine all pedogenic processes is not justified. Most pedologists would probably agree that some properties of the soil body quite unrelated to organic controlled processes, for example, the purely physico-chemical, are truly pedogenic. Not only properties associated with carbon and nitrogen or other organic cycles are pedogenic. In any case it is not possible to divorce organisms from the ecosystem as simply as that, and to assign to them an all-important role. One might immediately ask: what determines the organic cycles? Even some character in the genesis of which organisms do play a part may persist for a very long time under a change of environment. An example is a thick calcareous hardpan, formed as a result of the downward movement of calcium, the mobility of which has been chiefly determined by the presence of carbon dioxide from root-respiration. All properties in whose development organisms play a part are not equally easily destroyed. In soil genesis organisms are important only because they are associated with factors of climate, relief, parent material, etc. The assumptions concerning aggrading and degrading surfaces are also overdrawn. While Nikiforoff's discussion of them is quite a pedological contribution, the significance of which should not be underrated, the conclusions from special, and extreme, cases cannot be applied generally as has been attempted. Pedology as a science has been plagued with problems of degree, as witnessed by the abject and inevitable failure of multicategorical soil classification systems. In dealing with the significance of aggrading and degrading surfaces in soil genesis we are dealing, essentially, with questions of degree. Very much depends upon the rate of loss or deposition relative to the rate of processes of soil formation. At some stage the effects become insignificant. Relatively stable conditions can be general over a very large area, in fact, continental. The best documented example is that demonstrated by Prescott and his colleagues for Australia (Prescott, 1931; Bryan, 1939; Whitehouse, 1940; Crocker, 1946; Stephens, 1946). Australia experienced a long period of stability from the late Cretaceous until the minor crustal movements initiated in the mid-late Tertiary. During this time the continent was reduced to a peneplain, on which lateritic soils were extensively (continentally) developed. Features of the old lateritic characters, especially the sesquioxide crusts and pans, are widely preserved in current soils, reflecting parts of their past evolution.

Conclusions Concerning Soil Evolution and Polygenesis

Neither the theoretical arguments, among which there are inconsistencies, nor the practical examples that have been proffered for demonstration, make a convincing case for autogenic soil evolution. No doubt as time passes changes occur, or may occur, in the environmental soil-forming factors, and these inevitably result, if of sufficient magnitude, and within the potentialities of the preceding ecosystems, in a sequence of variations which might be regarded as soil evolution. The extent of such evolution is essentially determined by the character and succession of the environmental changes. In so far as the soil characters resulting from any combination of soil-forming conditions are irreversible,

or but slowly reversible, they will persist, to varying degrees, under the new conditions. All too frequently pedologists approach soil genesis from the starting point of a hypothetical uniform parent material being suddenly presented (soil-formation time zero) to the influence of the other pedogenic factors, and from which soil formation proceeds under uniform conditions until maturity (equilibrium) conditions are approached. Unfortunately, nature is not quite so accommodating, and most soils are polygenetic. Soils and landscapes develop continuously and simultaneously, under the influence of changing or fluctuating environmental factors. In this connection Bryan's and Nikoforoff's stress on aggrading and degrading surfaces is especially cogent. So also is Tansley's (1935) idea, accepted by Rode and others, that the basic natural unit is the ecosystem, from which portions like soils and vegetation can never be entirely divorced.

The foregoing conclusions, if correct, place important restrictions upon the general applicability of Jenny's approach.

CONCLUDING STATEMENT

The Value of the Functional, Factorial Approach

An assessment of the merit of the functional, factorial approach to the problems of soil genesis has been purposely deferred until the question of soil evolution and polygenesis had been considered. We can now ask: Are there fundamental and theoretical weaknesses in Jenny's approach which make it of doubtful or of little value? Are the problems of achieving sufficient functions or thoroughly reliable sequences insurmountable, or, even if we obtained them, are the interactions so important that we would still be unable to solve the fundamental equation? Is the approach, for one reason or another, purely of philosophical value? Such questions are easily put. They are not so easily answered. But because it seems to me that they are important, some answers will be attempted.

In the first place, there is no doubt that Jenny's "carefully reasoned and well balanced treatise," as Bryan (1946) has described it, has considerable philosophic value—a value which is well expressed in the influence it has had on current pedological thinking. It has a completeness, a unity, a simplicity, which are very satisfying to the scientist attempting to rationalize the complexity of nature. It has prompted new types of questions and probings into the genesis of soils and related subjects, and has thus opened up entirely new possibilities. If this were its sole worth its scientific importance would be enormous. Its value is not restricted to its philosophic merit, however, even though a number of difficulties of both a practical and theoretical nature do place a severe restriction upon the general application, and nature proves more complex than imagined.

Among the purely practical difficulties those of determining exactly what the independent factors are in each case, their measurement, and their expression in a numerical index, are very prominent. An equally great practical difficulty is that of determining the parent materials in polygenetic soils. The problem here is to define the state of the system at the initiation of the current combination of the independent environmental factors. To all intents and purposes it confines the application to monogenetic soils, that is soils which are entirely the product of the current environmental factors.

Yet it is not sufficient to discard Jenny's approach merely because it is not generally applicable or, without some modification, even specifically applicable. It must be either challenged on theoretical grounds, or be tried in practice and found wanting before it can be cast aside.

No theoretical reasons convincing enough to challenge the basic ideas have yet been advanced, even allowing that a modified treatment of the time factor may be desirable. Many practical problems, however, have theoretical implications, in that they are essentially the result of the limits imposed by the definitions necessary in considering the soil as a system. The determination of the state of the system at t_o, for example, is essentially a problem of this type. There are others. Whether one looks upon it as a practical or theoretical limitation with the functional method, the assumptions concerning uniformity of the necessary environmental factors, in space and in time, are what determines the value of the derivations. The conditions of uniformity apply to *pedologic significance*, that is, a factor is uniform in any ecosystem under consideration as long as the variation within it does not have significance in soil formation. What is the pedological importance of variation of a specific amount in any factor or combination of factors? It was shown in the earlier discussion of the vegetation factor that differences in the independent factors could have great pedologic significance if they had a selective effect upon organisms. There is every reason to believe that subtle variation in the other inde-

pendent factors may also have significance in soil genesis. The pedologic significance of a specific variation in any of the factors will depend essentially upon the combination of climate, parent material, relief, and organisms, and the time-state of the system; that is, general limits of uniformity cannot be set, for they depend upon the particular system. It must be freely admitted that in studies concerned with soil formation, including the sequences and functions collected by Jenny, assumptions of uniformity have usually been far too loose, and for this reason many functions are suspect. A notable exception was the uniformity of parent materials so carefully established by Marshall and Haseman (1942) in their pedogenic studies.

The fact that it is only possible to apply the functional, factorial approach with absolute certainty to monogenetic soils means that functions and reliable sequences can only be developed from a very limited part of the soils of the world. However, if instead of considering the whole soil body, the totality of properties, only those properties which have limited permanence, in themselves, such as soil nitrogen, are investigated, many polygenetic soils might also be included.

Within the monogenetic soils, and the polygenetic soils which might in certain circumstances be considered, "special cases" where suitable variability of a factor, or factors, is associated with the required uniformity of the remainder are very rare. The opportunity, therefore, for acquiring basic relationships between soil properties and specific soil-forming factors is much more restricted than Jenny apparently originally believed. These limitations do not necessarily make the approach less important, however. They enhance it to the extent that reliable functions and sequences are incomparably more valuable, because of their scarcity.

Although Jenny suggested that the fundamental equation might be solved, he did not demonstrate how. In practice neither he, nor anyone else, has gone beyond simple algebraic functions. The solution of his equation seems to be no nearer than before. One difficulty would seem to be that even were it possible to assemble a large number of functions, they would all relate to particular combinations of specific states of four of the five factors: parent material, climate, topography, organisms, and time. Indeed, when one considers that many of these factors can be further subdivided, as they include independent elements, and further, that there may be other independent factors to be added, then the possibilities for combination become tremendously great, and indeed are tremendously great in nature. The chances of assembling sufficient functions to solve the fundamental equation then seem remote, quite apart from questions of mathematical feasibility. An example of how a function determined under one combination of factors may vary from one under a different, but somewhat similar, combination is well demonstrated by the comparison of the nitrogen and organic matter contents of Colombian and North American soils having environments with equal annual temperatures and annual moisture values (Jenny, Bingham, and Padilla-Saravia, 1948). The Colombian soils have nitrogen and organic matter contents several times higher than those of North America. This is hardly to be wondered at in view of the fact that there were differences in the parent material, biotic, climatic (despite the mean values) and, possibly, time, factors.

Little can be said at present upon either the success or failure of the functional factorial approach to soil formation in practice. Application of the method has been found difficult because of the complexity in the field, and has not been adequately tried. Indications are that it is very promising in establishing trends even where conditions are not ideal. For example, although in the earlier comparison of nitrogen levels quoted the Colombian soils were so much higher than the North American soils, there were some important similarities of the nitrogen-climate relationships in the two environments. The nitrogen-climate surface constructed for the Colombian soil had the same shape as that which Jenny (1930) had previously established in the Great Plains area, despite the difference in magnitude of the constants in the two equations. Up to the present very few functions or sequences have been accumulated, and most of these have been from investigations by Jenny and his associates. It is highly desirable that workers elsewhere test their conclusions, and extend similar investigations to other combinations of the soil-forming factors. A highly significant and convincing application of the approach has recently been demonstrated by Hallsworth and his associates (1952). The real value of the method (apart from its philosophical merit) can only be assessed when many more functions and sequences, determined as accurately as possible, with the minimum of assumption, and with the reasonableness of necessary assumptions satisfactorily established,

have been accumulated. Because of the difficulties of finding the necessary combination and uniformity in nature, and the practical and practical-theoretical difficulties already outlined, the sequences and functions will in general be both short and approximate. Such data, however, could not only indicate the most important trends, but could also provide the foundation on which knowledge of pedogenesis will advance to new propositions and generalizations. To what degree the basic data will be adequate for this task will depend first of all upon the number and diversity of special cases in nature capable of providing the necessary relationships. The search for and investigations of these cases appear to be amongst the foremost tasks presently facing pedology.

Should the special cases be insufficient, or the results for various reasons be inconsistent, the value of Jenny's approach will prove to be mainly philosophical. But even a few reliable quantitative relationships will help pedology to shake off some of the heavy burden of deductive reasoning and speculative conclusions of causality. There is good reason to hope for more. In the past a qualitative type of natural history approach, completely lacking in rigor, where so many of the hypotheses and concepts are based upon cyclic arguments, so many just flatly stated opinions, has been the alternative. Unless it is possible to accumulate sufficiently accurate relationships for the simple cases, how can the more usual complex situations be understood?

There are a number of highly significant consequences of Jenny's philosophical and theoretical approach which have not been touched upon. This is not the place to deal with them at any length, and mention, therefore, will be made of only two of them which seem especially worthy of serious consideration. The first is that as the soil body occurs as a geographical continuum, its individual properties will vary continuously in response to variations of the soil-forming factors. Is there, therefore, such a reality as a soil class with absolute limits? The question is complex and an attempt to answer it here is not intended; but it is important, because it throws a theoretical challenge at the whole basis of multicategorical soil classification systems, and at the practicability and value of soil correlation. The second is that no correlation which "possesses general validity" can be expected between two soil properties except where the value of one is uniquely determined by the value of the other (e.g., percentages of inorganic and of organic matter). This means, if true, that a function between two dependent soil properties is never likely to have more than local significance, and throws theoretical doubt upon the value of many types of soil investigation. Similar doubts can be cast upon the usefulness, in other than a local sense, of climatic indices empirically derived from correlations between soil group boundaries, or vegetation boundaries, and climatic values. This does not mean that correlation between two dependent soil properties is not a valuable tool in soil research. It merely means that there are limits to its usefulness. It might be used, for example, to determine what soil properties are uniquely related, or the range of variability over which correlations can be achieved. Thus one of its most valuable uses could be to determine the pedogenetic significance of variability among the soil formers. In so far as this could reduce the assumptions in the functional approach, the method of correlation would be a valuable ally to the latter.

In the reviewer's opinion it is the ecosystem, and not Jenny's soil system, or Major's vegetation system, that is the best theoretical unit. Many theoretical difficulties, such as the substitution of environmental climate, relief, and organisms, for soil climate, soil relief, and soil organisms, dissolve if one considers the ecosystem and not the soil system. The importance of the ecosystem is well illustrated by considering the parent material factor in polygenetic soils—probably the most widespread soils. What determines the *effective* disseminule factor at any new t_o during the development of these soils is not the old soil but the soil vegetation system at the specific t_o. The importance of the broader system becomes obvious also as soon as one considers the reasonableness of the type of assumptions necessary in the functional approach. Many, perhaps most, assumptions can be assessed only by consideration and utilization of facts pertaining to the ecosystem as a whole. That is, use must be made of the judgments of the closely related natural science disciplines of climatology, geology, and geomorphology, ecology and biogeography, organic evolution, and history, in its widest sense.

It might be expedient to divide the ecosystem into soil systems, vegetation systems, etc., for purposes of study, but the ecosystem remains the best system for theoretical consideration. For this reason, in addition to both the practical limitations,

previously mentioned, and the possibilities of interactions, solution of the fundamental equation is not likely. The functional, factorial method will probably achieve most by stimulating new questions and new orientations for ideas, and by providing reliable information of the trend of soil formation under a limited variation of certain conditions. It will thus give a better basis for (1) the interpretation of more complex situations, and, perhaps more important, (2) investigations of the soil-forming processes. The method, although far more rigorous than alternative approaches, does involve numerous assumptions, and is not therefore entirely free from subjectivity.

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THE INFLUENCE OF THE NERVE IN REGENERATION OF THE AMPHIBIAN EXTREMITY

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INTRODUCTION

REGENERATION is the replacement of lost parts by growth of new ones. It is observed widely among animals and plants and consists of the insensible, continuous replacement of cells, the renewal of damaged tissues, and the reproduction of organs, organ systems, and in some animals even entire regions of the body. The capacity to regenerate is highly limited in some forms and very extensive in others, and varies with age, being most extensive in the early embryo, which shows remarkable powers to renew damaged parts by the rearrangement and reorganization of available materials and by regrowth.

The process of regeneration has recieved considerable attention from students of growth and development, since it emphasizes that regulated developmental capacities do not terminate with embryonic development but continue into the adult, albeit in a modified and restricted manner. In the experimental study of regeneration a number of forms have been favored because they show particularly well the ability to regenerate one or another part of the body or appendages. Coelenterates, flatworms, earthworms, starfish, and crustaceans are the important invertebrate ones; and the urodele amphibians (salamanders and newts), the frog tadpole, the lizard, and some of the fishes are the vertebrate representatives.

The regeneration process bears many resemblances to embryonic development and yet shows remarkable differences from it. Both the similarities and the differences have been the subject of considerable research. A striking similarity of regeneration to embryonic development is the accumulation in the region of injury of cells which have the disposition and appearance of the mesenchymal cells of the embryo, concentrations of which give rise to various organs. Moreover, the rapid growth of this cellular mass and its later structural and functional differentiation resemble closely embryonic formations in general appearance, arrangement, and developmental sequence. Yet regeneration cannot be equated with embryonic development, for the reason that the tissues which give rise to the new growth have a biological history and environment different from that of the embryo. The regenerate arises directly from and develops in intimate relation with adult structures, and there is no barrier between the new formation and the old tissues. The histology of the regenerate emphasizes the continuity and interrelation between adult and regenerating tissue, the former passing gradually into the latter with no absolute line of demarcation between the two. This intimacy of morphological relation between the regenerate and the adult tissue has demanded that study of the process of regeneration be made against the background of the anatomy and physiology of adult tissues.

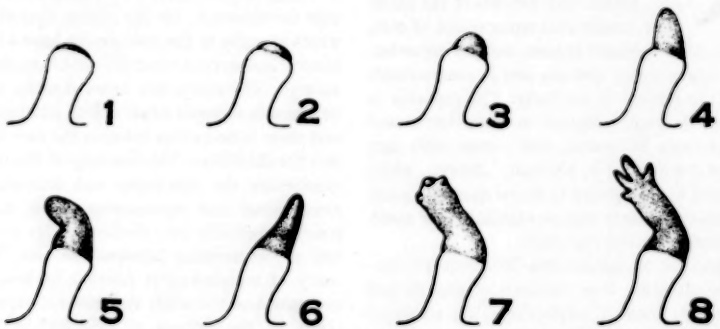
These relations with adult structures distinguish regeneration from embryonic development; they are various and, perhaps, more striking than the resemblances. The regenerate throughout its development is nourished by the adult blood stream, and there is no apparent obstacle to free exchange between the two, unless it be endothelial in nature. Thus the new growth is perfused continuously with substances of adult origin and importance. Adult epidermis adjacent to the wound area moves actively over the wound surface and forms the epithelial covering of the regenerate. The epidermis of the regenerate is thus continuous with the adult epidermis, although it is undoubtedly modified as it moves over the amputation surface, and therefore differs morphologically and physiologically from the adult epidermis (Godlewski, 1927; Orechowitsch and Bromley, 1934; Rose, 1948b; Singer, 1949; and others).

Regenerating nerve fibers arising from the interrupted ends of adult neurons invade the regenerate and are present within it in great numbers at all stages of development. Although not equivalent to adult axis cylinders, the regenerating fibers do have their trophic centers in adult cell bodies. Finally, it is most important to note in contrasting embryonic development and regeneration that the "cells of regeneration" have their origin either directly from adult cells by a series of changes in which the cell divests itself of differentiated structures or from primitive cells which lie among the adult tissues and are called forth by the injury (reviewed by Weiss, 1939; Schotté, 1940).

The influence which each of the adult tissues plays during regeneration has been the subject of considerable research. The present study reviews and evaluates the work on the role which only one of the tissues, the nerve, exerts on regeneration. However, discussion of the nerve influence cannot be abstracted from other aspects of the regeneration process. Consequently, mention will also be made in this review of other aspects of regeneration which are of importance in appreciating the nerve action. Furthermore, the discussion will be centered mainly on the nervous influence in regeneration in the Amphibia (particularly in the newt, salamander, and frog), since most work and theoretical evaluations have been devoted to this vertebrate group. References to the nervous influence in regeneration in invertebrates and other forms may be had in part from Schotté (1926a), Korschelt (1927), Locatelli (1929), Abeloos (1932), and others.

THE MORPHOLOGY AND HISTOLOGY OF THE REGENERATE. THE INNERVATION OF THE REGENERATE

In order to appreciate fully the nerve effect on regeneration, it is important to summarize at least briefly the major morphological and histological features of regeneration. Since most of the work on the influence of the nerve in regeneration has been done on the limb of the adult newt, it is appropriate to describe regeneration in this form. The major stages of regeneration of the forelimb of the newt, *Triturus viridescens*, are depicted in Figs. 1 to 8. The ensuing description of these stages is based upon personal experiences and upon the numerous reports of other workers (see, e.g., Götte, 1879; Fraisse, 1885; Barfurth, 1891; Wendelstadt, 1904; Glaeser, 1910; Fritsch, 1911; Schotté, 1926a; Hellmich, 1930; David, 1934; Thornton, 1938; Mettetal, 1939; and others).



FIGS. 1–8. STAGES IN THE REGENERATION OF THE FORELIMB OF THE ADULT NEWT TRITURUS VIRIDESCENS
Magnification, × 4.5

Within 12 to 24 hours after amputation (25°C.) the wound surface is covered by a thin epidermal sheet which migrates over the injured tissues from the edge of the wound. In ensuing days this sheet thickens greatly and instead of the three or four layers of cells of normal skin, the thickness is as great as ten or fifteen (Rose, 1948b; Singer, 1949). The tissues lying under the epidermis show profound changes. At first there is an essential wound reaction, and phagocytosis becomes evident, whereby tissue debris is removed. Phagocytic cells disappear within about two days, and connective tissue cells and fibers appear in increasing abundance under the covering epidermis. The fibro-

cellular connective tissue is well developed within 3 or 4 days after amputation and reaches its fullest extent by approximately 8 days. At this time and later it is gradually replaced by *mesenchymatous cells* (also called *blastema cells* and *cells of regeneration*) which accumulate in this region. The mesenchymatous accumulation (*blastema*) is quite pronounced by the tenth day following amputation, and the end of the stump is swollen, due to widespread edema of the wound area (Fig. 1). Sarcolysis of transected muscle and histolytic changes in other tissues are evident in the region immediately adjacent to the original wound before and during the time of accumulation of the cells of regeneration.

By two weeks the mesenchymatous accumulation is greatly enlarged, due to rapid mitoses; and the blastema, together with the underlying edematous tissue, imparts a dome-shaped appearance to the end of the stump (Fig. 2). The regenerate at this stage has been called the *early bud*. Growth is very rapid during the days immediately following early bud formation. The regenerate bud becomes somewhat conical within two days, attaining the stage of the *medium bud* (Fig. 3). The regenerate elongates further, and after about 3 weeks after amputation it is pointed (the *late bud*, Fig. 4). At this time cartilage is developing within the mesenchymatous mass and differentiation of skin glands has been initiated. Histogenesis of muscle and connective tissue begins shortly thereafter. The regenerate is supplied with a rich capillary network very early in development, and as the regenerate increases in size and differentiates, major vascular channels of arteries, veins, and their subdivisions are developed.

The first sign of morphogenesis occurs in the middle or latter part of the fourth week (25°C.) when a bend in the middle of the regenerate appears, representing the developing elbow (Fig. 5, dorsal view; Fig. 6, posterior view). Cartilaginous elements and articulating surfaces are observed histologically at this time. Another morphogenetic sign associated with elbow formation is the flattening of the distal part of the regenerate bud, a change which anticipates formation of the hand (Figs. 5 and 6). This stage is called the *hand* or *palette stage* (Schotté, 1926a). During approximately the fifth week of regeneration, digits appear for the first time as small swellings at the distal end of the growth (Fig. 7). At this time histogenesis is well advanced; major skeletal elements have been formed in cartilage; and muscle, connective tissue, skin, nerve, and blood vessels are well differentiated. Further development is an elaboration of structures already formed. In ensuing weeks, the fingers elongate as successive skeletal elements and the joints are completed (Fig. 8). The rate of growth of the regenerate in length and volume and the mitotic rate during regeneration have been described by Singer and Craven (1948). Once increase is initiated, it is very rapid and shows little change in rate before about forty days. Mitosis, however, reaches its peak (per unit volume of tissue) during the third week and then declines as differentiation sets in (see also Litwiller, 1939).

Except for the first two days after amputation, the regenerate is amply supplied with nerve fibers which grow out from the severed trunks of the amputation stump (Litwiller, 1938b; Singer, 1949; Taban, 1949). The fibers, as silver studies show (unpublished observations), pervade the entire regenerate throughout development. There is no tissue of the early regenerate which escapes the presence of the nerve (Singer, 1949). Thus the nerve is in a position to influence each region of the regenerate in quite early and critical stages of growth. In their growth, the nerve sprouts come to invade the epidermis in addition to deeper lying tissues. They are seen in great numbers among the epidermal cells, especially during the first few weeks of regeneration. While invading the epidermis, the fibers are naked and not accompanied by Schwann cells. During their wanderings in the epidermis many fibers are exposed at the surface, as epidermal cells are sloughed off. The epidermal innervation, rich at first, subsides with later development, and only an occasional fiber, as in the case of normal skin, is seen in the epidermis of the advanced regenerate. The epidermal innervation provides a nervous protection of the developing regenerate, since the growth is thereby rendered highly sensitive to mechanical stimulation (Singer, 1949).

Since the epidermis has been demonstrated to exert an important influence upon regeneration of the extremity (Tornier, 1906a, b; Taube, 1921; Schaxel, 1921; and Godlewski, 1927, 1928), the richness of the innervation of the epidermis during development provides a possible expression for certain speculations which have been advanced concerning the nature of the epidermal influence (Godlewski, 1928; Jefimoff, 1931, 1933; Orecho-

witsch and Bromley, 1934; Polezajew and Faworina, 1935; Polezajew, 1936a, 1945; Adova and Feldt, 1939; Rose, 1948a). These speculations attribute to the epidermis, among other things, an importance in histolysis of underlying structures (Polezajew and Faworina, 1935; Adova and Feldt, 1939; Peredelsky, 1940a, and a direct cellular contribution to the blastema (Godlewski, 1928; Rose, 1948a). Whatever the importance of the epidermis may be, the nerve is prepared by its invasion of the epidermis early in regeneration to evoke the epidermal influence (Singer, 1949). Perhaps the nervous influence is concerned particularly with mitosis of epidermal cells (Taban, 1949; see also Overton, 1950).

Mesenchymatous regions of the regenerate are still more richly supplied with nerve twigs, which wind among the cells in great numbers, branching repeatedly. These fibers are accompanied by their Schwann cells and show an intimate relation to the cells of the regenerate. As differentiation occurs, the fibers form functional connections with muscle, and the diffuse arrangement of nerve sprouts becomes reorganized into definite nervous channels which resemble closely in pattern and distribution those observed in the normal limb (Weiss and Walker, 1934; Litwiller, 1938b).

THE DEPENDENCE OF REGENERATION UPON INNERVATION

Todd in 1823 reported among other unique experiments and observations that interruption of the sciatic nerve profoundly affects regeneration of the hind limb of the salamander: "If the division of the nerve be made after the healing of the stump, reproduction is either retarded or entirely prevented. And if the nerve be divided after reproduction has commenced, or considerably advanced, the new growth either remains stationary, or it wastes, becomes shrivelled and shapeless, or entirely disappears. This derangement cannot, in my opinion, be fairly attributed to the vascular derangement induced in the limb by the wound of the division, but must arise from something peculiar in the influence of the nerve."

No further experiments are reported in the literature until the end of the last century, when interest in the relation between nerve and regenerate was revived as a result of studies on the influence of the nerve in embryonic development (see also reviews of Schotté, 1926a; Locatelli, 1929; Abeloos, 1932; Rose, 1948a). There was considerable disagreement then as to whether or not regeneration of various parts depends on the nerve; but it is now generally held, with only occasional objection, that a stump denervated at the time of amputation is incapable of subsequent regeneration. The appearance of a regenerate after denervation, reported by some workers, was due, as viewed from the present, to inadequate operative procedures or to inadequate maintenance of the denervated state. Barfurth (1901) reported regeneration of the tail in salamanders after he resected the spinal cord some distance rostral to the amputation surface. This operation, although it effectively interrupted descending fibers of the spinal cord, did not denervate the amputation area, since a segment of the cord as well as sensory and sympathetic elements remained at the surface. The experiments merely proved that descending connections from higher regions of the central nervous system are not necessary for regeneration of the tail.

Goldfarb (1909, 1911) also denied an influence of the nerve on regeneration of the forelimb. He held that there are no nerve fibers in the normal regenerate during its development—a belief that is without foundation in fact, as sections stained specifically for nerve fibers reveal (Rubin, 1903; Walter, 1919; Litwiller, 1938b; Singer, 1949). Goldfarb concluded that regeneration of a nerveless limb can occur eventually, albeit with some delay. But Wolff (1910), Schotté (1922c, 1926a), and others have shown that inception of regeneration in such delayed instances is due to reinvasion of the amputation stump by regenerating nerve fibers. Moreover, collateral fibers arising from adjacent spinal nerves may invade the limb in sufficient number to cause regeneration (Schotté, 1926a; Singer, 1946a). In most instances, however, regeneration of the denervated limb stump, at least in the adult, never occurs unless reamputation is performed (Schotté, 1926a). Apparently the skin, having completed its development over the amputation surface, effectively suppresses regeneration even after eventual widespread reinnervation of the stump—an explanation which may be drawn from the experiences of Tornier (1906a, b), Taube (1921, 1923), Nassonov (1936), Peredelsky (1940a), and others. Goldfarb also raised the possibility that the suppressing effect of denervation on regeneration, reported by others, might be due to extensive trauma during operation on the nerve and, therefore, not the result of nerve interruption. Yet, extensive trauma at the site of denervation of the limb stump (generally in

the brachial or lumbosacral plexus), even when it includes interruption of the arteries of supply of the limb, does not alter regenerative powers provided nerves are not transected (Todd, 1823; Hines, 1905; Weiss, 1925; Schotté, 1926a).

The nervous system was also considered unimportant for regeneration by Wintrebert (1903a, b, 1904), since regeneration of the hindlimb of salamander larvae appeared after removal of the lumbosacral spinal cord. It is now common knowledge that ablation of the spinal cord destroys the motor supply of the extremity and the central connections of the sensory supply, thereby paralyzing and rendering insensitive the limb. Although the motor component and functional sensory connections are destroyed by these means, the cell bodies of the sensory and sympathetic neurons and their peripheral connections remain intact. Consequently, a complete anatomical denervation was not accomplished in the operation employed by Wintrebert, and the conclusions drawn from the results are unjustified.

There are other and more recent views which question the importance of the nerve in regeneration. Uranovsky (1936) believed that the nerve is only of partial importance in regeneration of the axolotl's limb. In affirmation of Goldfarb's view, he held that the trauma which necessarily attends the operation on nerves is a more effective agent of inhibition of regeneration than the denervation itself. Schaxel and Schneider (1939) also denied an influence of the nervous system on regeneration, although Schneider (1940a, b) later modified this view and stated that the nervous system exerts a partial and indirect influence on the growth of the regenerate of the hind limb of the axolotl. According to him, the nerve influence is vasomotor in nature: interruption of vasomotor nerves impairs the nutrition of the limb; growth of the regenerate ceases when nutrient reserves are used up (compare with other views on the importance of the sympathetic nervous system discussed below).

In contrast to the above experiments, the view that regeneration of the amphibian extremity requires the activity of nerves received substantial support, so that there is little doubt at present of the necessity of the nerve for this growth process. Wolff (1902) was the first who reaffirmed the views of Todd concerning the importance of the nervous system for regeneration of the limb. He found that there was no regeneration of the limb of *Triton* after destruction of the sensory and motor supply at the time of amputation. Regenerative capacity reappeared with later reamputation, but only after a few months, by which time motility and sensitivity had returned—a fact which bespeaks reinvasion of the amputation stump by regenerating nerve fibers. Rubin (1903) successfully suppressed regeneration of the amputated forelimb of *Siredon* by resecting the brachial plexus. The same results were obtained on other forms by Hines (1905), Wolff (1910), and Walter (1911, 1919), who lent further support to the view that regeneration depends upon the nerve supply through additional experimental variations. Repeated analysis of this problem since then has reemphasized each time the marked nature of this dependence (Schotté, 1922a, 1926b; Weiss, 1922, 1925; Locatelli, 1924, 1925, 1929; Samarajew, 1939a, b).

Rubin (1903), although admitting the importance of the nerve for regeneration of the adult limb, denied it for larval regeneration (compare Goldstein, 1904). However, his experiments on larvae are subject to the same criticisms already directed at those of Wintrebert. Schotté (1923c, 1926a) demonstrated that regeneration of the larval limb requires the nerve, just as the adult one. Indeed, the effects of denervation of the amputation stump are more striking in the larva than in the adult. Not only is regeneration lacking, but the amputation stump is unable to preserve its morphological integrity in the absence of the nerve. It gradually regresses and is eventually resorbed completely unless nerves regrow into the stump (Schotté and Butler, 1941a, b; Butler and Schotté, 1941, 1949; Karczmar, 1946). Resorption of the nerveless stump occurs irrespective of the level of amputation, the prerequisites being injury by amputation and continued denervation. The rate of regression of limb stumps of various lengths and the factors controlling the rate have been studied by Schotté and Karczmar (1944). In their study of the nature of the resorptive processes of the denervated and amputated limb of the larva, Schotté and Butler (1941a, b) observed that there was no formation of a blastema, and that dedifferentiation of tissues of the amputation surface, which is set in motion by the injury to the limb and which normally subsides as the blastema is formed, continued to sweep proximally. Thus, dedifferentiation minus the controls imposed upon it by the developing blastema itself, as is ordinarily seen in the presence of the nerve, progressively converted the limb stump into a mesenchymatous mass whose final fate could not be fully ascertained

(Schotté, Butler, and Hood, 1941; Schotté and Harland, 1943a). Such a profound disturbance in the morphological integrity of an injured and denervated extremity has not been reported for the adult (see discussion of Rose's 1948a review by Butler); but atrophy, which is evident upon prolonged denervation of the adult limb, may be of the same kind, if not of the same degree, as the resorption described for the larval limb, although such an interpretation is not subscribed to by Schotté and Butler (1941a; see later discussion of the work of Singer and Craven, 1948, on denervation during later stages of regeneration).

In connection with the relation between the nerve and regeneration in the larval limb, the recent report of Yntema (1949) is of particular interest. He has reported that limbs which develop in the embryo in the absence of nerves are capable of regeneration in the denervated state. This being so, it would seem that the nerve dependence of regeneration of the limb arises as a result of association with the nerve during embryonic development. Stated otherwise, the limb in the course of its development becomes "addicted," so to speak, to the nerve contribution. Or, perhaps, in the absence of the nerve other growth factors, with which the embryo abounds but which may diminish later, are preserved in greater abundance in developing nerveless limbs and thereby compensate for the absence of nerve stimulation (see later discussion on the genesis of the nerve influence).

The relation between the nerve and regeneration of the limb has also been studied in the tadpole of the frog which, unlike the adult form, can normally regenerate a limb. Schotté and Harland (1943b) showed that denervation of the hind limb effectively suppresses regrowth. In contrast to these findings, Polezajew (1939) described experiments which do not support the view that regeneration of the tadpole limb requires nervous stimulation. Sectors of the limb transplanted to the abdominal region showed extensive regeneration within 9 to 12 days, during which time there was no invasion of the graft by nerve fibers from the locus of transplantation, as judged by histological study of silvered tissue sections. These results are at variance with those of Schotté and Harland unless one assumes that as a result of transplantation the regenerative capacities of the limb become independent of the growth-stimulating influence of the nerve (see also Rose, 1948a).

It is appropriate here to mention another experiment in which under certain conditions the limb of the newt is believed to show regenerative abilities in the absence of the nerve (Oliver and Rose, 1946). Treatment of the denervated amputation stump of adult animals with hypertonic salt solution, a method which evokes regeneration of the limb of the adult frog (see below), was followed by certain limited "regenerative" signs such as the formation of various cellular and fibrocellular concentrations at the wound surface. However, these wound reactions to the irritation of hypertonic saline hardly resemble regeneration of the innervated limb and, therefore, cannot be equated with the tissue responses to nervous stimulation.

The influence of nervous tissue on the regeneration of parts has also been studied in the tail of the adult urodele, the regeneration of which is quite pronounced and easily lends itself to direct observation. The innervation of the amputation surface of the tail differs from that of the limb. In addition to the motor and sensory contributions of spinal nerves and to sympathetic postganglionic fibers, fibers of the spinal cord are also injured and exposed in the transection of the tail. Moreover, the sympathetic chains consisting of segmental ganglia and their internodal connections of white and gray fibers are also interrupted and contribute, albeit in a minor way, to the complement of fibers at the amputation surface. The neural contribution of the spinal cord to the wound surface is substantial, and it is appropriate that attention was directed early to its possible influence upon regeneration. Destruction of the cord subjacent to the wound surface effectively suppressed regeneration (Godlewski, 1904a, 1904b), although the sensory and sympathetic contributions were presumably spared. These results are in contrast to those of denervation studies of limb regeneration, where it has been shown (see above) that destruction of the spinal segment of supply does not inhibit regeneration provided the sensory contribution is preserved (Wolff, 1902, 1910; Walter, 1911; Locatelli, 1924, 1925, 1929). Thus fibers whose cell bodies lie within the central nervous system appear necessary for regeneration of the tail but not for that of the limb. This apparent contradiction between the nervous requirements for regeneration of the limb and the tail are resolved by the quantitative studies to be described below.

The experimental results of Locatelli (1929)

on tail regeneration confirmed those of Godlewski, although she took issue with him in the interpretation of the results. She believed (see below), that the cord is only indirectly responsible for regeneration in that it is the source from which new spinal ganglia develop and that these in turn evoke regeneration of the tail. Lack of tail regeneration after cord destruction was also observed by Goldfarb (1909), but he attributed the results to trauma and not to loss of the cord. The previously mentioned experiments of Barfurth (1901) also sought to test the relation of the spinal cord to tail regeneration. Since a region of the cord was removed at some distance and not subjacent to the amputation surface, the interpretations which he drew concerning the activity of the cord in tail regeneration were not valid.

In emphasizing the dependence of tail regeneration on the spinal cord, it is of interest to recite here other experiences reported by Godlewski (1904a, b). Godlewski found that removal of a square piece from the axis of the tail, so as to include a segment of the cord but not to transect the tail completely, results in regeneration of supernumerary small tails from the anterior and posterior cut surfaces. The spinal cord is exposed at these surfaces, and not at the dorsal and ventral ones, and thus tail regeneration from these surfaces may be related directly to the presence there of the cord. On the basis of these experiments he pointed out that cases of supernumerary tail formation (Fraisse, 1885; Barfurth, 1901; Tornier, 1896; Terni, 1931) after the spinal axis is split or otherwise disturbed results from division of the spinal cord and the establishment thereby of two or more separate foci for tail regeneration.

There are still other experiences than these which serve to emphasize further that amphibian regeneration requires the presence of the nerve. This idea is implicit in the experiments and results of Singer et al. (1942–1949), Karczmar (1946), and others, although their works are devoted primarily to particular aspects of the nerve action.

The controversy over the importance of the nervous system in regeneration which has been reviewed above was undoubtedly due in part to differences in the manner in which the experiments were conducted. Yet, the controversy also reflects in an important way the information and views which prevailed some time ago in related fields, and, from the historical sense, must be evaluated against the scientific background of the times. Other controversies which then prevailed, such as that on the importance of the nerve in embryonic development, were in great part the source of the disagreement on the importance of the nerve in regeneration and undoubtedly colored the experimental procedures and conclusions (see below, section on genesis of nerve influence). Moreover, the neuron doctrine, as enunciated by Waldeyer, was not universally accepted at the beginning of this century. If one assumes that the axis cylinder is made up of a series of segmental contributions from successive Schwannian elements, then denervation has a different significance than if one agrees that the neuron is a genetic as well as morphological and functional unit. Another reason for the lengthy controversy was the general unavailability, except for the specialist, of techniques of heavy metal impregnation of nerve fibers. Silver techniques, such as are now generally employed, provide an accurate means of histological analysis of operative results. Inadequate information on the anatomy of the innervation of the extremities (see below) also contributed to differences in experimental results and conclusions. Indeed, in some instances operations on the salamander were planned and performed according to published information on the anatomy of unrelated forms.

In concluding this section on the dependence of regeneration upon innervation, one further point must be stressed: namely, that total denervation is not required in order that regeneration be suppressed. This fact and its significance have not been fully appreciated in the past. Yet it is implicit in the results of many experiments reviewed above in which regeneration failed to appear even though a residual nerve quantity was available at the amputation surface. For example, when, of the three brachial spinal nerves innervating the forelimb of the salamander, nerves 3 and 4 are destroyed, but nerve 5 is left intact, regeneration is as effectively suppressed as after total denervation (Weiss, 1925; Schotté, 1923d, 1926a; Singer, 1942a, 1943, 1945, 1946a, b, 1947a, b). Nor does regeneration of the tail ensue after cord destruction, although an intact sensory and sympathetic complement of nerve fibers is available at the amputation surface (Godlewski, 1904a; Locatelli, 1929). There are many other examples which might be drawn from the literature to show that subtotal destruction of the nerve may be as effective as total denervation. The explanation of the ineffec-

tiveness of a small residual nerve supply emerges from experiments described below on the importance of the quantity of nerve fibers for regeneration.

THE ROLE OF NERVE COMPONENTS IN REGENERATION

Before the dependence of regeneration upon innervation was fully resolved, another problem was raised concerning the role of individual nerve components (motor, sensory, and sympathetics) in the activity which nerves exert on regeneration of extremities. This problem had its origin largely from the classic experiments of Herbst (1901) on heteromorphic regeneration in the crustacean. In this animal, amputation of the eyestalk distal to the optic ganglion is followed by regeneration of an eye, whereas transection proximal to the ganglion yields an antenna instead. Herbst concluded from this that the optic ganglion exerts a specific morphogenetic influence on the development of the eye. He extended this generalization to regeneration in other forms and to embryonic development, and believed that sensory neurons control morphogenesis.

Wolff (1902, 1910) subscribed to the conclusions of Herbst as a result of experiments on hind limb regeneration of the urodele, *Triton*. Regeneration occurred when the lumbosacral spinal cord, which contains the cell bodies of motor neurons to the hind limb, was extirpated (see also Locatelli, 1924; Brunst, 1927); but, when sensory ganglia of that level were also ablated, regeneration was lacking. Thus, the sensory supply may not be dispensed with and appeared to be the all-important element in regeneration of the limb. These results and conclusions were reaffirmed by Walter (1911), a student of Wolff. However, on the basis of other experiences, Walter (1919) revised his judgment of the importance of the sensory component and implicated the sympathetics in the regeneration process, as one arm in a complicated reflex arc which includes the sensory neuron. If any part of this circuit, sensory or autonomic, is interrupted, regeneration does not occur.

Experimental analysis of the role of the sympathetics and other components was done by Schotté (1922b, 1923a, c, 1926c, d; see also Schotté and Butler, 1941a), who, in affirmation of Walter's suggestion, also attributed to the sympathetics an essential role in regeneration. The sympathetic system, by virtue of its known control over blood vessels and therefore nutrition of a part, was also considered important for regeneration by Weiss (1925) and Schneider (1940b). The idea that the sympathetic system is the essential neural element in the activity of nerves in regeneration prevailed until recent years. The reason that it was initially advanced and was later generally accepted in spite of some information to the contrary reflects in large part the great interest in the sympathetic nervous system during the second and third decades of this century. During this time many functions were attributed to the sympathetics which we now see have no basis in fact.

The all-importance of the sympathetic component in regeneration of the limb was denied by Locatelli (1924, 1925), the results of whose experiments confirmed, instead, those of Wolff and the early one of Walter. Regeneration occurred when the spinal cord which supplies the limb with its motor complement was removed, but when the sensory ganglia were ablated without injury to the cord, regeneration was in most instances effectively suppressed (Locatelli, 1924, 1925, 1929). In cases where regeneration occurred after ganglionic destruction, regeneration of the limb was attributed to regeneration of ablated spinal ganglia (Locatelli, 1924). The view that the brachial or lumbosacral ganglia regenerate after ablation is not substantiated in any instance of the histological studies reported by Singer (1943, 1946a).

In further support of the notion that sensory neurons are the essential neural elements for regeneration, Locatelli (1925, 1929) found that displacement of the ganglia and their proximal roots into the peritoneal cavity after destruction of the cord and interruption of possible connections of the sympathetic chain with the spinal nerves was followed by regeneration of the limb provided the distal connections of the sensory neurons were preserved. Locatelli (1924) noted further that the sympathetics were not responsible for regeneration of the limb; they could not be damaged when the sciatic nerve was cut because the sympathetics, she believed, took a separate course to the periphery along the blood vessels. This note on the separateness of the peripheral course of sympathetic and somatic fibers and the source of this information were not elaborated in her later definitive work (Locatelli, 1929). But, in confirmation of her belief, it has been shown more recently that in the newt, in contrast to the condition in the mammal, postganglionic sympathetic fibers for the fore-

limb do accompany the arteries peripherally and do not mingle with the somatic fibers of the spinal nerves (Singer, 1942b).

The somatic motor supply was considered the essential neural element in development by Godlewski (1904a) alone of all the workers. He stressed the observation that regeneration of the tail does not occur after removal of the cord from the amputation surface, even though undegenerated sensory fibers remained. Locatelli (1929) took issue with this interpretation of the influence of the spinal cord and attributed, instead, only an indirect importance to the cord. As noted previously, she stated that the cord is necessary in order that regeneration of spinal ganglia occur. The regenerated ganglia in turn are responsible for regeneration of the tail. The notion of ganglionic regeneration preceding tail regeneration is not in accordance with fact, since spinal ganglia appear only after regeneration of the tail is quite advanced (Duèsberg, 1922) and are therefore not available for the early initiation of growth. Consequently, the fact that destruction of the cord suppresses regeneration of the tail but not of the limb requires another explanation than that of Locatelli. One possibility is that different nerve components are required for each—in the one instance (limb), sensory or sympathetics are needed; and in the other (tail), elements of the cord are essential. A more likely possibility, based upon other experiences to be described below, is that the nerve effect is independent of the component of innervation. This explanation of apparent differences in component requirements has emerged from studies of the quantitative requirements of nervous tissue for regeneration of the extremity (see later).

It is apparent upon reviewing the literature that a number of reasons underlie the general disagreement on the importance of each component for regeneration. For one, the problem was not appropriately posed and consequently the experimental procedures and conclusions were based on unjustified premises. The question was generally asked, "Which nerve component is the important element in the nerve action on regeneration?" Thus the problem stated in advance that one or another nerve component is all-important for regeneration. Yet there was no reason to be found in the previous literature, other than the speculations of Herbst, to assume that one or the other component should be the sole agent of the neural influence on growth. Indeed, it is now apparent that all nerve fibers may be active in evoking the growth. Inadequate information on the anatomy of each component, particularly the sympathetics, was also a source of fundamental differences and of inadequate control of experiments. Finally, histological checks of the results of operations were seldom employed and in still fewer instances were staining procedures used which are specific for nerve fibers.

The problem of the influence of each nerve component in the regeneration of the urodele limb was reanalysed recently in *Triturus*, following a series of histoanatomical studies of the sympathetics and other nerve components of the forelimb (Singer, 1942a, b, 1943, 1945, 1946a, b, 1947a, b). Histological controls of the results of the operation were prepared by means of silver-stained sections through the limb and the entire body wall. The results showed that only the sensory one of the three nerve components (motor, sensory, and sympathetic) is capable of stimulating regeneration in the absence of the other two. The results conform to those reported by Locatelli (1924, 1925, 1929), Wolff (1902, 1910), and Walter (1911), and in part by others. From these results one might conclude, as those workers did, that sensory fibers alone have the quality of stimulating regeneration; or, stated otherwise, that sensory fibers differ qualitatively from motor and autonomic ones and alone are able to evoke a regenerative response from the tissues of the amputation surface. However, in the course of analysis, some evidence appeared which questioned the notion of such a qualitative distinction of fibers of the sensory component (Singer, 1943, 1945, 1946a). First of all, regeneration does not occur when the sensory contribution to the amputation surface is partially destroyed after thorough de-efferentation or even when some motor fibers remain in addition to sensory ones, such as when nerves 3 and 4 are cut out but nerve 5 is spared. Thus, the residual sensory complement is as ineffective a stimulus of regeneration as the normal motor or sympathetic supply. The ineffectiveness of a reduced sensory supply was first pointed out by Walter (1911), who considered briefly the possible importance of fiber quantity in limb regeneration.

Another reason for questioning the conclusion that only sensory fibers can stimulate regeneration is that regeneration of the limb may be evoked by a pure motor supply in experiments which allow

motor fibers of the ventral root to regenerate back into the limb after transection of the ventral and dorsal roots and ablation of the sensory ganglia (Singer, 1946; see also Schotté, 1922a). In the course of regeneration to the periphery, the motor fibers encounter under such circumstances more pathways in the degenerating peripheral stumps of the spinal nerves than they previously occupied. In consequence, the fibers branch repeatedly and tend to saturate the now vacated sensory pathways as well as those of the motor ones, as Litwiller (1938) described. In this way the limb receives more axis cylinders of motor nature than were present previous to the operation. The increased motor representation may be considered the neural agent of limb regeneration, and it would follow, therefore, that motor fibers also have the neural quality necessary for limb regeneration but are unable to express this quality when all other fibers are destroyed, since motor fibers do not normally exist in great enough quantity in the limb. Thus, the normal motor component may be deficient in quantity and for this reason be as ineffective in stimulating growth as a greatly reduced sensory supply.

There are, conceivably, other explanations why the regenerated motor supply but not the normal one can evoke growth, but these appear less important than the quantitative one, as subsequent discussion will emphasize. One such alternative explanation is that motor fibers as they regenerate acquire the quality of growth stimulation; another is that a period of nervelessness of the limb stump prior to reinnervation by regenerating fibers makes the tissues of the limb responsive in some way to motor fibers.

A third reason for questioning the conclusion that sensory fibers are qualitatively peculiar emerged from available anatomical information on the quantitative representation of fibers of each component within the limb. Counts of fibers of the dorsal and ventral roots of various vertebrates, including the frog, have shown that the sensory root contributes more than the motor one (Agduhr, 1933). Undoubtedly the sympathetic provides still less than the somatic motor one. Thus, quantitative superiority rather than qualitative peculiarity has emerged as a possible explanation of the sensory action in regeneration. Finally, to these three reasons must be added the many reported experiments in which extensive but subtotal destruction of the nerve supply suppressed regeneration (see above). In these instances a residual supply of presumably active fibers could not stimulate growth.

THE IMPORTANCE OF QUANTITY OF NERVE FIBERS

Except for some casual remarks in the earlier literature, the significance of which was established by experiments to be described below, the idea that the quantity of nerve fibers might be important in the nerve action on regeneration was not advanced until recently. Walter (1911) had noted briefly that quantity of nerve might be important for regeneration. Schotté (1922a) had analyzed the effectiveness of each of the brachial spinal nerves in regeneration and correlated the over-all diameter of the nerve with its capacity to evoke regeneration, an experimental procedure elaborated in further detail by Karczmar (1946). And Schotté (1922b) and Weiss (1925) had each suggested that intensity of regeneration might be related to nerve quantity.

Analysis of the relation between the number of nerve fibers and the activity of the nerve in regeneration was initiated with a study of the quantity of motor, sensory, and sympathetic fibers contributed by each of the three brachial spinal nerves to a selected amputation level of the upper arm of the newt, *Triturus* (Singer, 1946b). The quantitative analysis showed that there is a great disparity in the number of fibers contributed by each component to the amputation level and that the disparity becomes increasingly marked distally. The sensory quantity far exceeded that of the other two components, being almost four times the somatic motor number in the upper arm level and still greater in more peripheral regions. Sympathetic fibers were extremely few when compared to either of the other two components and the sympathetic:motor:sensory ratio was 1:6:22. This anatomical information about the number of nerve fibers comprising each nerve component may be correlated with the effectiveness of the component as a neural agent of limb regeneration. The isolated sensory component, alone of the three nerve components, can evoke regeneration in the absence of the other two; underlying this qualitative difference is the fact that the sensory supply is quantitatively most favored. The motor component, which in the absence of sensory fibers does not evoke regeneration of the limb, is represented by fewer fibers at the amputation level. And, finally, the sympathetic supply, the least conspicuous contributor to the innervation of the

limb, is at the same time the least effective growth-stimulating agent.

Employing the anatomical information of the quantitative contribution of each component of each brachial spinal nerve to the amputation level of the upper arm, it was then possible to destroy selectively various quantities of fibers at the time of amputation and to study the influence of the remaining number upon regeneration (Singer, 1946b, 1947a, b). The motor and sensory components of the three brachial spinal nerves were cut out in various combinations, so that the remaining fibers ranged from almost that number normally found at the surface of amputation to one representing almost total denervation. In those instances where the residual supply surpassed approximately one-half of the normal, regeneration invariably followed, irrespective of which brachial nerve or how much of each component was destroyed. Regeneration never occurred below a level of about one-fourth up to almost one-third the normal innervation number. Between these extremes of all positive and all negative instances a range of fiber values existed which yielded regeneration in some cases but not in others, the latter being more frequent in the lower values of the range. This fiber range was considered a threshold value, since above it regeneration invariably occurred and below it regeneration was always absent. The fact that the threshold was best defined loosely as a range of fiber values rather than a single specific quantity was attributed to a number of factors, important among which was individual variation in the response of regenerating tissues to the nerve. Also of great importance was the fact that the number of fibers estimated to remain after each operation represented only a statistical value based on averages obtained in the previous anatomical studies. The actual number of fibers at the amputation level in any individual case may be expected to differ from the mean, and the effect would be to increase the variance of the threshold value. In order to rule out the effect of such deviations from the mean, it would be necessary actually to count the residual fibers in the experimental limbs. Such a procedure would itself introduce other variables which appear at the moment to be of greater import than the one of fiber variation.

Another factor, the control of which might increase the precision of the description of the threshold, is that of differences in fiber size and activity. The assumption is implicit in the above procedures that all fibers are equal, since the fibers irrespective of differences in their size or activity in regeneration, were given equal values. It is more probable, however, that the fibers are not of equal effectiveness in stimulating regeneration (see below). If in a specific denervation one type of fiber is favored more than another, the threshold value may be higher or lower than for another denervation where the activity of the residual fibers is of a different order. However, since large numbers of fibers remained after even the most extensive partial denervation employed in the experiments, it is probable that with each denervation one is dealing with a population of fibers whose average activities are similar irrespective of the nature of the operation. Experimental evaluation of these speculations concerning the precision of the threshold range may allow more precise definition of the quantitative nerve requirements for regeneration of a part. As it appears now, the refinement should, however, alter in no important way the major conclusion derived above, namely, that nerve quantity or its counterpart, quantity of nerve activity, is important in the nerve effect on regeneration.

The results also resolve in large part the question of the activity of each component in regeneration and show that the fibers of all components are probably effective agents of growth, since the presence or absence of growth could not be correlated with the component supplying the residual fibers, but only with the total fiber number. The experiments substantiate the views previously drawn from anatomical and other considerations (Singer, 1943, 1945, 1946a) that the fibers contributed by the motor and sympathetic supply are quantitatively inadequate to evoke growth when isolated in the limb. Indeed, the lower limit of the threshold range surpassed the normal motor contribution and was much greater than the available sympathetic supply. The sensory contribution, on the other hand, was well above the threshold range, which accounts for its greater growth-stimulating potential. Yet partial destruction of the sensory quantity yielded a sensory complement as ineffective for regeneration of the limb as the normal motor or sympathetic supply (see also Walter, 1911). Therefore, it is reasonable to assume that all fibers irrespective of their origin have the quality necessary for the regeneration process. This assumption includes also the fibers of the central nervous system, since, as noted before,

the spinal cord is required for regeneration of the tail.

The analysis of nerve quantity and regeneration was extended to more distal levels of the limb: the mid-lower arm, the hand, and the fingers (Singer, 1947a). Threshold nerve requirements for regeneration appeared in each instance. Reflecting the importance of the quantity of nerve fibers, it was interesting and impressive in some studies of finger regeneration to observe that following a given operation on the brachial spinal nerves, the residual intact fibers should prove adequate for one finger and not for another. Thus, the fate—in terms of the presence or absence of subsequent regeneration—of one finger was independent of that of another and was fixed by the quantity of fibers available at the amputation surface, even though all fingers were amputated simultaneously and at the same level.

Support for the view that quantity of fibers is important for the expression of neural activity in regeneration may be drawn from other experiments as well. Schotté (1923d) early pointed out that the larger spinal nerves were more effective in stimulating regeneration than were smaller ones. No inference was drawn then concerning the importance of nerve quantity for regeneration except to relate alterations of rate of later growth of the regenerate with the size of the nerve destroyed. More recently, Karczmar (1946) has further elaborated the experiments of Schotté relating nerve size to its activity in the regeneration of the limb of the larval newt. He measured the cross-sectional area of each brachial spinal nerve in the brachial plexus in order to estimate the size of the neural contribution of that nerve to the amputation surface. He then transected one or more nerves and amputated the forelimb. His results showed that the nerves of larger size are more effective agents of growth and, consequently, that quantity of nerve fibers is important in the nerve action. However, he disagreed with the idea of threshold response and held instead that there is a graded response of the regenerate to increasing quantities of nervous tissue. This conclusion was based on the observation that among the positive cases there was a delay in the time of first appearance of the regenerate, the extent of which could be related directly to the residual undamaged nerve quantity. In the adult, delay in the first visible signs of regeneration at the amputation surface was noted by Singer (1946a, 1947a), who reported an occasional delay when regeneration occurred in the presence of a limited but quantitatively adequate number of fibers. The delay appeared to bear no obvious relation in frequency or extent to the number of fibers available for the growth. Reanalysis of this point with additional experiments in which various residual stimulatory quantities of fibers were available at the amputation surface substantiated these results (Singer and Egloff, 1949). In most cases there was no delay in appearance of the regenerate, but when it did occur it seldom extended beyond a week. Moreover, there was no evidence of a correlation between the extent of the delay or its frequency and the number of nerve fibers initially available at the amputation surface. The insignificant alteration in the time of first appearance of the regenerate of the adult limb was also pointed out by Schotté (1923d) and Weiss (1925) for the partially innervated limb of *Triton*. They noted, however, that the subsequent speed of the growth was often less than in the normal regenerate, and Schotté correlated the degree of slackening of later regeneration stages with the size of the nerve destroyed. In the studies of Singer and Egloff (1949) a delay in attaining successive stages of regeneration was also prevalent and was further accentuated in the later stages. But here again there was no parallel between the extent of nerve destruction and the degree of delay. Consequently, the graded responses reported for the larva do not obtain for the adult, where a threshold reaction is apparent. The disparity between the results in the larva and the adult are unexplained at the moment.

The demonstration that quantity of fibers is important in the action of the nerve in regeneration provides an explanation for the dependence of tail regeneration on the spinal cord and the dispensability of the cord for limb regeneration. The sensory and sympathetic components which remain after ablation of the spinal cord cannot supply the neural contribution necessary for tail regeneration; and yet, similar destruction of the cord of supply of the limb with preservation of sensory and sympathetic elements does not interrupt regeneration of the limb. Assuming that quantitative nerve control of regeneration also holds for tail regeneration, then it is apparent that the sensory component, although it satisfies the threshold needs of the limb, is quantitatively inadequate for tail regeneration. In the latter case the substantial fiber contribution of the cord to the amputation surface is required to exceed the threshold.

THE RELATION BETWEEN SIZE OF THE AMPUTATION SURFACE AND THRESHOLD NERVE FIBER REQUIREMENTS

The threshold number of nerve fibers required for the regeneration process showed marked variations for different regions of the forelimb of *Triturus*, with but one exception (Singer, 1947a). In that exception the upper and lower arm showed notable similarities in nerve requirements. The hand required fewer fibers than more proximal regions; and the digits still less with the following order of decreasing threshold requirements: digit 3, 2, 4, and finally 1. A number of explanations may be offered for the observed differences in fiber requirements of various regions of the limb. The possibility that threshold needs are determined by the amount of tissue to be regenerated must be ruled out by the fact that regeneration of the upper and lower arm requires similar quantities of fibers, although much less tissue is regenerated after transection of the lower arm than after amputation through the upper arm. By the same reasoning, nerve requirements are not established by the amount of tissue amputated, although one might imagine this to be so if one compared regeneration of the fingers alone, where the fiber requirements are greatest with the largest digit and least with the smallest one. Nor can differences in nerve requirements be attributed to a progressive change in the intrinsic nature of the nerve fibers as they course distally within the limb, for one would expect a parallel change in nerve requirements which certainly does not obtain between the upper and lower arm levels.

A more reasonable possibility to explain the differences in demands upon the nervous system made by various levels of the limb seems to reside in demands which tissues of the amputation surface make upon the nerve (Singer, 1947a, b). It is known that tissues in the immediate vicinity of the wound supply the cells of regeneration and thus give rise to the blastema (Hertwig, 1927; Thornton, 1942; Butler and O'Brien, 1942). The nerves in their action on regeneration must, therefore, be concerned especially with these tissues and not with distant parts of the limb. Consequently, differences in the wound region of various levels of the limb must underlie differences in nerve demands. There may be differences in the competence of tissues of various levels to respond to the nerve stimulation. For example, the level of the limb which requires the least fibers might be the most competent one in its response to the nerve. Another possibility, which indeed received experimental support, is that the amount of tissue injured during amputation establishes the nerve requirements for regeneration. This seems a reasonable hypothesis, since the size of the wound surface varies from one region of the limb to another, being large in regions with a great area of transection and small in parts in which less tissue is exposed upon injury. Since the essential events of regeneration occur in the immediate vicinity of the injury and not in distant parts, the area of the surface of amputation may be taken as a measure of the amount of tissue subjected to the regeneration process and to the nerve action (Singer, 1947b). The area of amputation was measured for each level of the limb previously employed in the quantitative studies, and when threshold requirements were expressed in terms of unit area, it was observed to be relatively constant from one region of the limb to the next except for regeneration of the hand. The widely separated regions of the upper arm and fingers had remarkably similar requirements per unit amputation area. Therefore, the threshold nerve requirements for regeneration of each region are established not simply by the physiological responsiveness of the tissue to the nerve, but also by the amount of tissue exposed through wounding to the regeneration process. The lower and upper arm have similar total requirements, even though separated by a distance from one another, because their cross-sectional areas are similar. Unlike other regions, the hand requires fewer fibers per unit area of exposed tissue. The lower threshold needs of the hand cannot be explained at present, but they may conceivably be due to peculiarities in the anatomy and physiology of this region of the limb. There may be a greater abundance here of the tissues which give rise to cells of regeneration; or these cells may arise from greater depths than elsewhere. Perhaps the tissues of the hand are, indeed, more responsive (competent) to the nerve stimulation. An anatomical peculiarity of the hand which may explain in some yet unknown way its smaller requirements is the abundance of skeletal tissue seen in cross-section, as contrasted to that of other regions of the limb.

THE ROLE OF THE INDIVIDUAL NERVE FIBER

Although the demonstration of threshold nerve activity clarifies the role of nerve components in regeneration, it raises at the same time other

pertinent questions. Particularly important among these is the question concerning the part which the individual nerve fiber plays in the quantitative stimulation of growth (Singer, 1946b; Singer and Egloff, 1949). The individual fiber is the source from which the nerve influence emanates, whatever the nature of this influence may be. The stimulating quality of the fiber must either proceed from the axis cylinder alone or in conjunction with the investing sheaths. It can hardly emanate exclusively from the sheaths. The sheath of Schwann is present in denervated stumps but then regeneration does not occur. The myelin sheath appears to be of even less importance than the neurolemma, since regeneration of the limb can be called forth by regenerating nerve fibers which have as yet not acquired a myelin sheath. This was particularly evident in the experiments on the stimulation of regeneration of the limb by regenerating motor fibers (see above).

The site of origin within the neuron of the stimulating quality is also worthy of some discussion. Regenerating nerve sprouts which arise from the transected nerve fibers and invade the tissues of the wound surface and of the regenerate may be the only parts of the neuron to have this growth-stimulating quality. However, the quality may be common to non-regenerating axis cylinders as well as regenerating ones, including in the latter case both the nerve sprouts and the proximal cylinders. Perhaps the activity of the nerve which is essential for regeneration arises from all parts of the neuron. Or, as seems most reasonable now in the light of modern information about the neuron, the quality may have its origin in the "trophic" center of the neuron, the cell body, and thence pervade indiscriminately the neuronal processes, impressing itself finally upon the growing tissues.

Each of the nerve processes in contact with the regenerating tissue is probably active in stimulating the growth. But, as stressed above in the quantitative studies, the contribution of the individual fiber is inadequate unless it is supplemented by that of many other fibers acting in unison with it. Stated otherwise, the essential quality which the neuron contributes to regeneration does not exist in a single neuron in an amount adequate enough to satisfy the requirements for regeneration of the limb or its parts. Thus, the activity of the individual neuron receives expression only in the collective activity of many neurons, each contributing, according to its ability, to meet the threshold set by the tissues of regeneration.

Although it seems reasonable from the results of studies on the role of nerve components in regeneration to assume that all nerve fibers have the capacity to stimulate regeneration of the limb, it does not necessarily follow that the fibers are all equal in this activity (Singer, 1946b; Singer and Egloff, 1949). Some fibers are probably more effective agents of growth than others. As noted before, fundamental differences in the morphology and physiology of neurons may be the basis of such speculative differences in potency of fibers. Fibers of large caliber may be more effective than smaller ones. Thus, postganglionic sympathetic fibers, which are very small in diameter, may be a relatively ineffective source of the nerve stimulation, in contrast to the larger sensory and somatic motor ones. There are many other possibilities. A fine collateral or even a major branch of the neuronal axis may not provide as important a stimulus as the main stem. The potency of a given fiber may reflect the number of times the main axis cylinder has branched. For example, two fibers of equal caliber, one having arisen after repeated branching of the parent stem and the other being less removed in branching from the primary axis, may not be of equal activity in stimulating regeneration. The latter fiber may represent more nearly the functional capacity of the neuron in evoking peripheral growth, whereas daughter fibers of repeated divisions may carry the stimulatory agent in diluted fashion, since it is known that the sum of the calibers of branched fibers exceeds the parent one (Eccles and Sherrington, 1930, and others).

A point concerning individual nerve fiber activity which was raised recently (Singer and Egloff, 1949) stresses the probable existence of such differences in potency of parent fibers and their branches. The number of fibers counted in a cross-section of the limb represents the quantity initially available for regeneration at the time of amputation. However, as regeneration of the extremity proceeds, it is quite evident from our knowledge of nerve fiber regrowth after injury that each transected axis cylinder present at the amputation surface must sprout repeatedly, thus multiplying the original number of fibers by some unknown but presumably considerable factor. Irrespective of the initial nerve quantity at the amputation surface, the extent of multiplication of the sprouts must be such as to saturate the tissue of the wound surface and the regenerate (Litwiller, 1938a, b).

In this way the number of fibers for a unit volume of tissue should approach a norm whether or not partial denervation of the stump had been performed. Since results of quantitative studies show that the presence or absence of regeneration can be directly related to the number of fibers initially available at the amputation surface, branching must not, therefore, excessively enhance the expression of the neurons in growth. Instead, the parent fibers must be looked upon as the source or fountainhead of the peripheral growth effect. The number of these fibers must establish in large part the quantitative influence. Nerve twigs, by their repeated divisions, serve to carry the nerve influence to all regions of the regenerate.

Differences in activity of fibers in regeneration may also be related to differences in the amount or kinds of investing sheaths of the axis cylinders. A further difference may lie in the component of origin of the fiber. Perhaps fibers of one component are more effective than fibers of another (Singer, 1946b, Singer and Egloff, 1949). For example, the individual sensory fiber may be a more or less effective agent of growth than the motor fiber.

Assuming that the nerve fibers differ in their relative contributions to regeneration—an assumption which appears reasonable at the moment—then the importance of quantity of nerve fibers takes on additional significance. The stimulatory value of a given nerve quantity is, after all, an expression of the combined activity of the contributions of the individual fibers, as discussed above. If fibers differ in their potencies, then the threshold requirements for regeneration should vary according to the composition of the nerve quantity. For example, in instances where the nerve fibers comprising a given quantity are individually not of great potency, the number of fibers needed to exceed the threshold should be higher than in cases where fibers of greater capacity are active in the regeneration process.

The above theoretical description of the relation between threshold fiber requirements and the capacities of the individual nerve fiber emphasizes both the individuality of the nerve fiber in the regeneration process and the importance of the collective activity of nerve fibers in stimulating the growth. Once the differences among fibers are elucidated, it may be possible to assign values to the fibers and to treat the threshold requirements in terms of the growth-stimulating potential of the nerve fiber population, rather than in terms of the number of fibers. This would allow a more accurate definition of threshold requirements. At the moment, fiber number remains the closest approach to an evaluation of nerve activity in regeneration, and the predictability of the results with nerve quantities derived from different sources suggests, as noted previously, that one is dealing in each instance with a population of fibers having a similar over-all variation in types of growth-stimulating fibers.

INDUCTION OF REGENERATION IN THE ADULT FROG BY AUGMENTATION OF THE NERVE SUPPLY

The information which has emerged from study of the influence of the quantity of fibers in regeneration of the forelimb of the newt explains at least in part the loss of regenerative capacity which the limb of the frog suffers at the time of metamorphosis (Singer, 1950, 1951). The extremity of the tadpole can regenerate, and this regenerative power requires the presence of nerves at the amputation surface (Schotté and Harland, 1943b). Presumably in the tadpole as in the salamander, threshold nerve requirements must also be satisfied, although experiments on this point have not yet been performed. The loss in regenerative capacity of the limb during metamorphosis is rather abrupt (Barfurth, 1894; Wintrebert, 1904; Polezajew, 1936; Forsyth, 1946; and others) and appears to progress in a proximal to distal direction (Schotté and Harland, 1943c), although one occasionally observes signs of a limited and spontaneous regeneration of the limb after its amputation in the postmetamorphic frog (Polezajew, 1936; Thornton and Shields, 1945; Singer, 1951). Moreover, the loss is not an irrevocable one, for regeneration has been successfully induced after mechanical or chemical irritation or following other experimental alterations of the amputation surface (Polezajew, 1936a, 1945, 1946, 1947; Gidge and Rose, 1944; Rose, 1942, 1944a, b, 1945, 1947).

There are a number of theories to account for the decline and loss of regenerative power (Naville, 1926a, b; Guyénot, 1927; Liosner, 1930; Borssuk, 1935; Polezajew, 1935, 1936b, 1946, 1947; Weiss, 1939; Peredelsky, 1940b; Schotté and Harland, 1943a; Gidge and Rose, 1944; Barber, 1944; Rose, 1944a, 1948a; Forsyth, 1946; Umanski, 1946; Goodwin, 1946). In general, these theories agree that the ability to regenerate is lost because of some change which occurs in the tissues or the relations between tissues at the time of metamorphosis. The

nature of the change is not known, but it is considered by some to mean "increased differentiation" of the tissues at the time of metamorphosis and loss therewith of the capacity to "dedifferentiate." Others simply infer that there is a decline in "organization" of the limb to account for the loss in regenerative powers. Recently, the idea was advanced that important changes involving the nerve or relation of the nerve to other tissues might partly underlie the loss in regenerative capacities (Singer, 1950, 1951). Perhaps the changes, whatever their nature may be, are such as to render the available nerve fibers numerically inadequate to evoke regeneration. Thus, the nerve quantity would not meet the threshold requirements for regeneration of the metamorphic and postmetamorphic limb and would, therefore, be an ineffective stimulus of growth. Should threshold requirements be satisfied by some experimental means, then the limb would no longer be refractory and regeneration would ensue.

Experiments were devised consequently to augment the nerve quantity normally available at the amputation surface with an additional supply in the hopes of satisfying threshold demands (Singer, 1950, 1951). The sciatic nerve and its tibial and peroneal branches were dissected free of the hindlimb and the distal cut ends were rerouted successively under the skin of the flank, side and stump of the previously amputated forelimb to the amputation surface. The proximal connections and origin of the nerves were not interfered with, so as to avoid or minimize Wallerian degeneration. In contrast to the very occasional and highly limited regeneration observed in the controls, experimental animals showed extensive regeneration of the forelimb, reminiscent in many ways of that observed in the urodele amphibian. Two to three weeks after operation there was an enlargement at the peripheral end of the forelimb stump, with the formation of an early to medium regenerate bud. Growth and differentiation were rapid in the following weeks, and structures resembling those of a forelimb appeared, although heteromorphosis was frequent. Digits appeared in a number of instances, and these ranged from well-formed structures to irregular knobs on the distal ends of the regenerate.

In the light of the successful induction of regeneration in a nonregenerating form, it is appropriate now to speculate further upon the causes for the loss in regenerative power during metamorphosis. The changes responsible for this loss may lie within the nerve itself, within other tissues which must necessarily respond to the nervous influence in order for regeneration to occur, or within both. Some histogenetic or histochemical change may occur within the tissues, rendering them relatively refractory to nerve stimulation. In such an event, threshold nerve requirements would rise and the demands surpass the capacity of the available fibers, thereby rendering the normal nerve quantity as ineffective a stimulus of regeneration as the quantity remaining after the extensive though not total denervations described before for the newt. The changes may, however, be in the neuron itself, rendering it a less effective agent of growth. In that event the threshold number of fibers required to evoke growth must necessarily rise commensurate with this decrease in potency of the individual neurons, although the requirements of the tissues may remain the same. More nerve fibers are required because of the increased ineffectiveness of the individual fiber. If the decline in potency of the neuron continues beyond a particular level, then the nerve effectiveness decreases below threshold needs and regeneration does not occur. Loss of regeneration power during metamorphosis may, perhaps, be due to a combination of these factors, namely, increased refractoriness of tissues to nerve stimulation, and secondly, decline in the growth-stimulating potency of the neuron. There are other possibilities, as well, which should emerge upon further analysis of this problem.

ASPECTS OF THE REGENERATION PROCESS WHICH ARE INFLUENCED BY THE NERVE

Although the importance of the nerve for regeneration in amphibians can no longer be doubted, exactly what the nerves do in the regeneration process requires considerable future elucidation. The information which is available at present suggests the lines along which further studies may be made. Among these observations is the important one that there is no accumulation of mesenchymatous cells at the amputation surface to form a blastema when denervation is performed at the same time as amputation (Schotté and Butler, 1941a). In the larval salamander, as noted previously, dedifferentiation sets in and sweeps proximally, and the entire stump is progressively converted to mesenchymatous tissue which is destroyed and resorbed (Butler and Schotté, 1949). In the denervated limb of the adult, as in the fully

innervated one, there are initial reactions of the stump to injury, namely, epidermal overgrowth and appearance of inflammatory changes and of a fibrocellular cap at the wound surface. But the later appearance of a mesenchymatous concentration which typifies normal regeneration does not occur after denervation. The nerve is therefore essential for the initiation of regeneration or, as Butler and Schotté (1941, 1949) have termed it, the "phase of accumulation" of cells of regeneration and the conversion of the cellular accumulation into a true blastema with morphogenetic potencies (see also Rose, 1948a). According to Schotté and Butler (1941a), the nerve acts "as a regulating agent of the complex cellular interactions which normally are responsible for regeneration." How the nerve acts during the phase of accumulation of the blastema is not known. It may cause directly the mobilization of mesenchymatous cells, and there may be as many explanations of the mobilization as there are speculative sources of origin of the blastemal cells (see review on origin of regenerating tissues, Schotté, 1940). In this connection, the possibility was suggested by Guyénot and Schotté (1926a) that Schwann cells migrating along growing axons which rapidly invade the amputation surface may be a major source of the cellular accumulation. The idea was based on the well known fact that Schwann cells migrate out along regenerating fibers and accompany them in great abundance and are therefore evidently present at the wound surface in the early days after amputation.

In the course of mobilization of the cells, the nerve may not be involved with processes of dedifferentiation, since dedifferentiation occurs unchecked in the amputated and denervated larval limb (works of Schotté and of Butler, lit. cit.). Instead, the nerve action in mobilization may be the collection and organization of mesenchymatous cells to form a true blastema (Butler and Schotté, 1949; a "true blastema" of regeneration being defined in the sense of Butler and Schotté, 1941, "as a group of cells which form at the tip of a limb a definite recognizable aggregate, normally developing into a regenerate.") The nerve action may be multiple and complex and involve a number of aspects of the early regenerative phase, or it may be single, involving only one of a number of important steps in the growth. In the latter event, the effect of withdrawal of the nerve influence would be to block regeneration by interrupting an important link in the sequence of events necessary for regeneration. Butler and Schotté (1949) have advanced an interesting possibility in speculating upon the nature of the nerve effect during the early phases of regeneration of the larval limb. They propose that autolysis, which occurs during dedifferentiation of tissues and which appears to go unchecked when the nerve is divided early during regeneration, is normally controlled in some fashion by neuronal activity.

As the regenerate develops, there are changes in its response to denervation. In the larva, denervation of the stump of the forelimb up to about seven days after amputation (at 20°C) results in immediate cessation of growth and later gradual resorption of the regenerate and then, even, of the stump itself (Schotté and Butler, 1944; Butler and Schotté, 1949). However, denervation after approximately seven days following amputation does not interrupt growth and development. The nerveless regenerate continues to grow and the various parts of the limb differentiate. Thus, once the larval regenerate reaches a certain phase in its development, it has become a "well-determined regenerate" (Schotté and Butler, 1944) and no longer requires the nerve activity. The stage when the blastema becomes independent of the nerve is the one immediately following mesenchymatous accumulation—a stage equivalent to the early or medium regenerate bud of the adult limb described above (Figs. 1 and 2). Schotté and Butler speak of this phase of regeneration as one of imbalance, "during which the essential processes of induction undoubtedly occur, resulting in the transformation of the 'accumulation blastema' into a morphologically determined regenerate." Thus the nerve is essential for the "induction phase" as well as the "accumulation phase." After the induction phase the nerve is no longer required for growth and differentiation of the larval regenerate, except in the functional sense.

The dependence of regeneration of the adult limb on the nerve shows certain notable similarities, and yet important differences, to that of the larva. Schotté (1923b) believed that nerves exercise their action throughout the regeneration of the adult limb—a conclusion also reached by Weiss (1925). Singer and Craven (1948) reanalysed this problem, with quantitative studies of regeneration of the adult limb in which length, volume, and mitotic changes were recorded after denervation. Denervation before 13 days (at 25°C)—a stage comparable to the larval one of 7 days—was followed by almost complete cessation of cellular

division and interruption of growth in length and volume. Indeed, there was invariably a resorption of the regenerate and therewith a loss in volume and length. On the thirteenth day in a few instances, and invariably in cases denervated thereafter, the response of the regenerate to denervation changed in part. Increase in length, and differentiation of the regenerate now occurred in the absence of the nerve. However, the length of the denervated regenerate was substantially less than that of the normal control, an observation which conforms to similar ones made by Brunst (1927); and the structure of the regenerate was often atypical, as Schaxel (1921) and Brunst (1927) have reported before. But increase in volume of the regenerate never occurred following denervation at any stage of regeneration. Indeed, in most instances, a notable loss in volume appeared in the days immediately following interruption of the nerve, a fact which Schotté (1926a) also noted and which was early recognized by Todd (1823) when he stated in his admirable work: "And if the nerve be divided after reproduction has commenced, or considerably advanced, the new growth either remains stationary, or it wastes, becomes shrivelled and shapeless, or entirely disappears." In the regenerate of the adult limb the mitotic rate was also diminished after denervation at all stages of development, but less profoundly in later stages than in early ones. Similar studies on changes in volume and mitotic rate after denervation during later stages of regeneration of the larval limb, such as those described above for the adult animal, were not undertaken by Schotté and Butler (1944, 1949) in their studies of the influence of denervation on regeneration of the larva, and consequently a strict comparison between the results of denervation in larva and adult is not possible here. Study of their plates suggests that growth in volume of advanced regenerates subsequent to denervation may not be as profoundly influenced in the larva as in the adult.

The marked importance of the nerve for growth in volume throughout regeneration of the adult limb resembles that of the nerve for the maintenance of volume of adult tissues. Loss in volume is one of the most striking of the atrophic changes which the adult limb suffers after denervation (compare Weiss, 1925). This observation is important since it shows that there evidently are similarities between the nerve action in regeneration and in the maintenance of adult tissues. The dependence of maintenance of volume of peripheral structures on the nerve is thus not a nervous event unique for regeneration but is rather a basic expression of nerve activity and of the response of tissues to withdrawal of the nerve.

The atrophic loss in volume of adult limbs after denervation is most often explained as a functional loss, i.e., as atrophy resulting from the loss of movement. Function undoubtedly plays an important formative role in the development of structure, according to the description of Roux and others (see, for example, Brunst, 1927, 1932; Schotté, 1940). Yet functional loss attendant upon nerve interruption cannot provide the explanation of volume loss in denervated regenerates, particularly that of early stages of development in which differentiation has as yet not appeared. In these cases volume effects are observed even before movements of the regenerate can occur. Moreover, a thoroughly paralyzed, although sensory-innervated stump can regenerate fully (Brunst, 1927; Singer, 1943; Sidman and Singer, 1951; and others), although functional defects such as the secondary fusion of skeletal elements and the appearance of other abnormalities occur frequently (Brunst, 1927, 1932). On the basis of this observed direct action of the nerve upon the volume of the regenerate, it is possible to reevaluate the results of nerve loss upon the morphological integrity of the fully developed adult limb. Undoubtedly functional disturbances yield atrophic changes, as is shown in the wasting of fully innervated but immobilized limbs. But in addition to these effects one needs to inquire whether a "non-functional" effect of nerves upon tissues of the limb may also obtain, reminiscent of the nerve activity during regeneration. Such a direct effect of the nerve in the maintenance of the volume of peripheral structures has been alluded to in the literature before, and the experiments of Tower (1935, 1937) on muscle atrophy serve as one important example. Other examples of the dependence of morphological integrity of a peripheral structure upon the nerve, other than in the functional sense, are described below.

The disparity between changes in volume and length following denervation after 13 days of regeneration is of other interest, in that they show that these two processes, which ordinarily go hand in hand, may be separated and proceed independently. Regenerates denervated after 13 days showed loss in volume while at the same time there was increase in length. How growth in

length can occur in the absence of increase in volume or, indeed, during volumetric resorption, has been speculated about by Singer and Craven (1948).

Morphogenesis and histogenesis were also observed to be independent of the nerve when denervation was performed after 13 days of regeneration, and proceeded despite the above described volume limitations (see also Schotté, 1923b, 1926a; Guyénot and Schotté, 1923; Weiss, 1925). These results with the regenerate conform to the view, which is now widely accepted, that nerves do not control morphogenesis and histogenesis in the embryo (see, e.g., Hamburger, 1927, 1928, 1939). This view has a considerable history of controversy behind it both for embryonic and regenerative development. The controversy, particularly as it bears upon the subject of this review, is evaluated separately below. In summary then, length increase, morphogenesis, and histogenesis may occur in the absence of the nerve, even though volume increase is suppressed and the mitotic rate profoundly depressed. The neural independence of length increase and differentiation after a certain stage of development of the regenerate must be qualified by the statement that it occurs only after an adequate previous history of active growth of the regenerate under the influence of the nerve as well as of other factors. Unless these influences have played upon the responding tissues at the amputation surface over a long enough time so that various events in development have occurred, length increase and differentiation of the regenerate will not ensue after denervation. The nature of the physical and chemical state of the blastema which finally allows it to grow in length and to differentiate in the absence of the nerve is not known. The interesting thought that the effect of delayed denervation is determined in part by the mass of the regenerate was advanced by Schotté (1923b, 1926a) and Guyénot and Schotté (1923). Polezajew (1934) also expressed the belief that once the blastema is large enough, it can proceed in growth and differentiation as an independent self-differentiating system. This would mean that the blastema would acquire certain qualities by virtue of new quantitative levels attained. Thus the interacting systems (cellular and intercellular) would increase in complexity during regeneration until at certain levels of interaction capacities of various sorts would emerge as new phenomena. Applying these thoughts to length increase after denervation: once an adequate number of cells is present, movements of these cells in relation to one another can occur more readily and the cells be rearranged to form a longer structure. In the case of differentiation, length increase may be envisioned as a necessary concomitant of axial differentiation such as is typical in the development of skeleton and muscle.

The response of cellular division in the regenerate to denervation also deserves further comment. As noted before, there appears to be some primary relation between mitosis and nerve activity, since the mitotic rate responds immediately to withdrawal of the nerve impulse (Singer and Craven, 1948). The nature of the mitotic response, particularly when denervation was performed up to the seventeenth day after amputation, was in most instances a sudden outburst of mitotic activity beyond that of normal, followed by a rapid decline of the mitotic number to a very low level. The sudden outburst of mitotic activity preceding mitotic decline is reminiscent of the excessive response of other tissues to loss of their nerves of supply. Fibrillation of striated muscle immediately after denervation and the initial and marked increase in the response of denervated smooth muscle and glands to chemical mediators are examples of this phenomenon (Cannon and Rosenblueth, 1949). The tissues respond excessively in the direction of nerve stimulation almost as though to compensate for loss of the nerve. Such increased response of tissues to loss of the nerve has been generalized and is now known as The Law of Denervation (Cannon and Rosenblueth, 1949). The outburst of mitotic activity observed in the regenerate immediately after denervation and preceding mitotic loss bespeaks, by analogy with the results of studies of other denervated tissues, a primary relation of the nerve to cellular division in the regenerate (see also Overton, 1950).

There are other aspects of the regeneration process which are also considered by some to be influenced by the nerve. Schotté (1923d) and Weiss (1925) raised the possibility that the speed of regeneration is controlled by the amount of nerve, since the rate is decreased by partial denervation. Baserga (1931) suggested that the quantity of fibers remaining after partial denervation affects the volume of the regenerated limb. Karczmar (1946), as noted previously, expressed the belief that the amount of nervous tissue controls the time of initiation of growth and that partial denervation delays the first appearance of the growth. Moreover, he speculated that the rate of

growth and the final size of the regenerate are controlled by the nerve.

Recently, studies have been reported on the relation between quantity of nerve fibers and speed of regeneration, time of initiation of regeneration, and final size of the regenerate (Singer and Egloff, 1949). When regenerates developed under the influence of a limited, although threshold-adequate, quantity of fibers, aberrations in time of onset, rate of growth, and final size of the regenerate invariably appeared. Morphogenesis was sometimes disturbed, too, although in general a fully formed limb developed (see also Schotté, 1923b, 1926a). However, the severity of these aberrations was independent of the degree or the pattern of nerve destruction, and so the extent of disturbance in rate, time of initiation, final size, and morphology of the regenerate are not determined by the extent of nerve damage. Brunst (1927, 1932) also described aberrations in the development of the skeletal elements resulting from nerve disturbance. These took the form of fusions or reduction in number of carpals. Such developmental variations in the regenerate have also been produced by means other than nerve injury. They occur when regenerating animals are subjected to various environmental conditions or operations and are therefore not specific to nerve alteration alone (Ellis, 1909; Schmalhausen, 1925; St. Hiller, 1928; Polezajew and Faworina, 1935).

In summary, available evidence seems to show that nerve fibers are important in the early processes of regeneration, and have to do with the accumulation of mesenchymatous cells to form a blastema at the amputation surface. Once the regenerate has reached a certain size, differentiation and increase in length continue in the absence of the nerve. But volume increase, at least in the adult animal, cannot proceed at any stage of regeneration without the nerve, and mitosis is always profoundly affected by nerve interruption. Finally, the nerve appears not to control the growth characteristics of the regenerate, although disturbances of the nerve supply may yield heteromorphic regenerates.

THE GENESIS OF THE NEURONAL QUALITY ESSENTIAL FOR REGENERATION

In the preceding pages it has been noted repeatedly that the dramatic influence of the nerve in regeneration is not confined to the adult animal but is observed in the larval form as well (Schotté, 1923c, 1926; Schotté and Butler, 1941a; Butler and Schotté, 1941; and others). Such neuronal activity has not, however, been traced back beyond the larval stage of development. On the basis of experiments on the effect of destruction of nerves and nerve centers on the development of the embryo, it is generally assumed that embryonic nerve does not exert an influence on development similar to that observed on regeneration. This being the case, the genesis of the nerve influence necessary for regeneration must occur between the embryonic and early larval periods. In order to evaluate fully these thoughts concerning the time when the nerve first becomes active in stimulating directly the growth of other parts, it is appropriate to review the experimental evidence and results of studies on the embryo which have given rise to them. Another important reason for reflecting upon these embryological studies is the fact that historically the problem of the influence of the nerve in regeneration of parts arose from considerations of the role which nerves play in embryonic development.

During the middle of the last century, the view was prevalent that the nervous system controls the development of other body structures, since anomalies in births were often observed in which deficiencies in development of body musculature were associated with derangements in the nerves of supply. The correlated disturbances were explained on the basis that the growth and differentiation of muscle and other structures depended upon the condition of the nerve supply and that disturbances in the development of the latter were reflected in alterations in the former (see, e.g., Neumann, 1903; Korschelt, 1927). The idea of such a formative role of nerves was upheld and supported by a number of workers, and various interpretations of how the nervous system affected growth were advanced. These views were questioned by many workers as a result of numerous investigations (see, e.g., reviews by Korschelt, 1927; Weiss, 1939).

Removal of the embryonic nervous system of young anuran embryos does not suppress muscle differentiation (Schaper, 1898; Goldstein, 1904), and limb buds can develop quite normally in the absence of nerves (for example, Harrison, 1904; Hamburger, 1927, 1928; Hamburger and Waugh, 1940). Histogenesis of muscle is completed, although the muscle degenerates secondarily unless

functional connection with the nerve is made. The culturing of differentiating myoblasts in vitro has also emphasized the independence of muscle fiber development of the nervous supply (Olivo, 1928; Goss, 1933; and others). Differentiation of structures such as bone and joints (Fell and Canti, 1934) can also proceed without the nerve. Although functional connections with the nervous system appear unnecessary for morphogenesis and histogenesis, the early nervous system does play an important part in inducing or initiating the development of other parts "by virtue of its organizing power as an embryonic field district" (Weiss, 1939). Examples of such inductions are the lens by the optic cup, and the nasal area and auditory placode by the brain.

The view that nerves do not have a formative influence in development except in a functional sense during the later growth of differentiated parts of the embryo is now generally accepted, and there is little basis for questioning this conclusion. Likewise, as already touched upon and to be described more fully below, nerves do not control the morphogenesis and histogenesis of the regenerate. However, a growth-stimulating potential of the embryonic nervous system, such as one observes for nerves in regeneration during the postembryonic period, has not been recognized for the embryo and is also assumed not to exist. If such a disparity in activity of embryonic and adult nerve is real, then it must follow that the nerve, at some stage in the development of the animal, acquires qualities which it did not possess before or which it possessed in only a limited amount; or that developing tissues, once connected to the nerve and subserving nervous expressions, acquire a dependence upon the nerve for their maintenance or their growth during regeneration.

However, another possibility emerges if one reevaluates the facts of experimental embryology. This alternative theory would hold that nerve fibers of the embryo are also active agents of growth of other parts and that therefore the quality of growth stimulation and maintenance, which can be readily demonstrated in the postembryonic form, is a fundamental activity of neurons of all stages of development. It is well known that embryonic tissues have remarkable growth capacities and that the juices of the embryo as employed in tissue cultures contain factors which stimulate growth. Assuming that these factors are many, as seems reasonable, it is hardly likely that removal of one or another of them should seriously affect further development. Applying these thoughts to the nervous system: continued development of a part after destruction of the nerves of supply, for example, of the limb bud, does not mean that embryonic nervous tissue is normally not active in the growth of that part. The embryo may be so richly endowed with factors to insure continued growth even after loss of the neural factor that destruction of the nerve does not substantially alter subsequent growth. This possibility may be elaborated further in order to explain the demonstrable importance of the nerve in postembryonic growth by noting that as development proceeds and as factors which control growth presumably decline in abundance and effectiveness, there is a relative increase in importance of those factors which remain. Withdrawal of a single factor or a combination of them, which previously did not alter the developmental course, may now result in alteration or loss of the capacity to grow. In this way, the nerve effect may emerge in increasing importance for growth so that after embryonic life withdrawal of the nerve affects the growth capacities of a part since the remaining agents of growth or others which have emerged in development (for example, important endocrine factors, Hall and Schotté, 1952), when they are not supplemented by nerve activity, constitute an ineffective stimulus. Dependence of regeneration upon the nerve is now observed in the larva and in the adult, although in the embryo, regeneration or completion of development does not suffer when the nerves of supply are destroyed. In summary, this reinterpretation of the results of embryological studies on the role of the nerve in development would hold that nerves may have the quality to stimulate growth in embryonic as well as in later life. But this activity of embryonic nerve is covered by the many other growth agents with which the embryo abounds, and only emerges later as a clear and effective growth force when these other agents subside sufficiently and are no longer able to satisfy the threshold requirements for growth stimulation without the presence of the nerve. That neurons may be important agents of growth in the embryo may also be drawn from the recent review and evaluation by Hamburger and Levi-Montalcini (1950) of certain aspects of neuroembryology, including the influence of one neuron on the development of another as well as on non-

nervous structures (see also Hamburger and Waugh, 1940).

THE QUESTION OF SPECIFIC MORPHOGENETIC ACTIVITY OF NERVES

In previous pages the conclusion was stressed, based on certain lines of evidence, that regenerates are capable of differentiating in the absence of the nerve once they have reached a certain size or stage in their development. Thus, the nerve does not control the nature of the structure developed, although it is important for the events preceding structural differentiation. It was also recorded that other characteristics of the developmental process, such as the time of initiation of growth, the rate of increase, and the final size of the regenerate are not determined by the nerve, although they may be profoundly altered by nerve damage. These views have not been shared by all students of regeneration and, indeed, there is a considerable history of controversy over whether there are morphogenetic nerves. The controversy led to many interesting and important experiments and conclusions which deserve detailed treatment and evaluation.

The idea that nerves may determine the nature of the regenerate is a direct extension to regeneration of the notion, which was so prevalent among embryologists at one time, that nerves have a morphogenetic action during development (see preceding section). This view was embodied in the conclusions of Herbst (1901) on regeneration of the eyestalk in crustaceans (see above). Wolff (1902, 1910) believed that for amphibian regeneration there are nerves with limb-forming qualities and others which have qualities specific for the development of other parts (see also Walter, 1914). Godlewski (1904a) suggested that the spinal cord might play a formative role in regeneration of the tail.

Locatelli (1924, 1925, 1929) ardently supported the view of a formative role of nerves in regeneration and attributed to individual spinal contributions the responsibility for the development of particular parts. She considered nerve 18 specific for regeneration of the hindlimb of the salamander; and absence of certain parts of the regenerate, when they occurred after nerve operations, she attributed to partial loss of fibers of this nerve. Baserga's (1931) experimental studies of the influence of specific spinal nerves on regeneration of the hindlimb do not agree entirely with those of Locatelli. Although removal of spinal ganglion 18 suppressed regeneration after amputation through the lower leg, growth proceeded after amputation through the upper leg or through the knee. Removal of ganglion 17 in addition to 18 was required to suppress regeneration in the two latter regions. Baserga therefore concluded that there is no specific relation between the nerve and determination of the regenerate. In passing, it may be noted here that this difference which Baserga described in the importance of ganglia 17 and 18 at various levels of the limb can be explained by the conclusions drawn above as to the importance of the quantity of fibers in regeneration. At one level, removal of ganglion 18 evidently decreases the fiber quantity below the threshold value, but at another level the threshold value is maintained despite the loss of ganglion 18. It is interesting that loss of the 18th ganglion is felt less at more proximal levels than at distal ones, an observation that may be explained by the fact that proximal regions are richer in fibers of various nerves than distal ones. Differences in the effectiveness of spinal nerves may even be observed at the same level of the limb, as has been shown in the preceding pages in regard to the importance of quantity of fibers in finger regeneration. Certain digits often failed to regenerate when a particular one of the brachial spinal nerves was transected. For example, sometimes the anterior digits did not develop when nerve 3 was interrupted, or the posterior ones when nerve 4 was transected. These results were shown to be due to quantitative predominance of innervation by the particular nerve in that digit, and there was no need at all to invoke explanations of specific morphogenetic control by this nerve, as the view of Locatelli would demand. If enough fibers were present after partial denervation, regeneration of the digit ensued.

Locatelli (1924, 1925, 1929) sought support for her theory of specificity of nerve action in the results of unique experiments which she performed, evoking regeneration in heterotopic positions by deviation of nerves of the hindlimb. The sciatic nerve or nerves of the lumbosacral plexus were freed of adjacent tissue and transected, and the distal end of the proximal stump was placed under the skin at or near the base of the limb. After a number of weeks or months a small limb developed at the site of the deviated end of the nerve. To the nerves which undoubtedly caused the formation of the supernumerary limb Locatelli attributed a limb-specific quality, in support of her view of a specific morphogenetic activity of nerves. Regeneration of a heterotopic structure

when limb nerves are deviated to another site was not obtained in *Siredon* by Schaxel and Schneider (1940) or by Schneider (1940a).

Locatelli's results were substantiated and further elaborated by Guyénot and Schotté (1926a, b), who showed that although an ectopic limb is developed when the nerve is deviated to the base of the limb or shoulder, deviation to the crest of the back and deviation of sacral nerves to the tail yield respectively crest and tail formations, and not limbs. These workers pointed out that the nature of the structure evoked was determined by the territory to which the nerve was deviated and not by the nerve. The nerve itself was non-specific in its action and merely effected a response from the locus of its action, without in itself determining what structure appeared. On the basis of these and other experiences, these workers and their associates (Guyénot, 1927; Guyénot and Schotté, 1927, 1926b; Bovet, 1930; Valette, 1926, 1929; Schotté and Hummell, 1939; Guyénot, Dinichert-Favarger, and Galland, 1948; Kiortsis, 1951) elaborated the notion of "territoires de regeneration"—a view which conforms with similar ones which emerged at that time from the work of experimental embryologists and which received expression in the theoretical and experimental works of Gurwitsch (1922) and Weiss (1922, 1926) on "morphogenetic fields" of regeneration and development (reviewed by Weiss, 1939; Needham, 1942). According to the field theory, the body is made up of regions whose developmental capacities are specific and limited, for example, to limb, tail, or snout formation. When growth occurs within this field, however it may be evoked, the structure formed is typical for that region. The fields are weakest at their periphery, where they fade into one another. When the entire field is excised, regeneration of that part does not occur (Schotté, 1926b; Valette, 1926, 1929; Efimov, 1944).

Interpretation of nerve action in regeneration as a non-specific, stimulatory one rather than as a specific morphogenetic effect was also reached through other experiments on regeneration, as has already been noted above for quantitative studies of the influence of the nerve in finger regeneration. Moreover, it has been pointed out by a number of workers in the past that morphogenesis and histogenesis do not require the nerve or that the nerve does not control the size, rate, and time of initiation of growth (Weiss, 1925; Schotté, 1923b, 1926a; Singer and Egloff, 1949). The factors which control the nature of the regenerate must be sought for in the tissues and tissue interrelations which give rise to the growth. The nerve is but one of the "realizing" factors which allow these parts to express themselves by regeneration. This is underscored by the fact that heterotopic supernumerary formations can also be evoked by various agents (Nassonov, 1936, 1938; Kuzmina, 1940). Also, regenerate buds of a given stage of development when transplanted to another body site develop according to the region from which removed, even though presumably innervated by nerves of the heterotopic site (Kurz, 1912; de Giorgi, 1924; Weiss, 1927). And, segments of limbs transplanted elsewhere regenerate typical parts (Weiss, 1923; Schaxel, 1934; Juge, 1940), although supplied by fibers of the site of transplantation. Similar conclusions regarding the non-specificity of the nervous stimulation of regeneration may also be drawn from the work of Luther (1948a, b), in which regeneration of the tail was observed after irradiation with x-rays and replacement of irradiated skin of the tail with unirradiated skin of the foot. And, more recently, Weiss (1950) has shown that fragments of central nervous tissue of urodele larvae transplanted into the dorsal fin along with a limb, will innervate the latter structure and supply the nervous activity necessary for regeneration upon amputation of the limb.

THE RELATION BETWEEN IMPULSE CONDUCTION AND THE INFLUENCE OF THE NERVE IN REGENERATION

A primary problem in the analysis of the influence of the nerve in regeneration is the mechanism whereby the neuron affects the regeneration process. At the present time there is no experimental information definitely to resolve this question. There are, however, certain areas of speculation and experimentation which delimit the problem of the nature of the neural mechanism and define the lines along which it may be attacked in the future. These thoughts are considered in the following pages of this review.

First of all, the question must be asked whether impulse conduction, for which the nervous system is primarily specialized and which is the mechanism of the reactions and the adjustments of the organism to its environment, is also the mechanism of nervous influence in regeneration. In answer, there are reasons for believing that the nerve action in regeneration bears little or no relation

to impulse conduction, as ordinarily conceived, and may therefore be viewed as a distinct neuronal phenomenon (Sidman and Singer, 1951; see also Hamburger and Levi-Montalcini, 1950, p. 159).

One reason is that the influence of nerves on regeneration flows from both motor and sensory fibers in a peripheral direction, being impressed upon the tissues and blastema at the wound surface. The centrifugal direction of flow of the "trophic impulse," if it may be called such (Parker, 1932), conforms to the direction of impulse conduction in motor fibers but not in sensory fibers. In the latter, impulses have their origin in the peripheral twigs and are conducted thence through the main axis of the neuron to the synaptic connections of the cell within the central nervous system. Thus, the direction of the nerve effect in regeneration is independent of the direction of impulse conduction. Parker (1932) and his students have stressed this reverse relation between the "trophic" effect of sensory neurons on taste buds and lateral line sense organs of fish and the direction in which impulses are conducted.

One might argue that an antidromic impulse, arising within the central nervous system and travelling across the synapse in a reverse direction and from there out along the sensory branch to the ultimate twigs, is the agent to which the effectiveness of sensory fibers in the regeneration process might be ascribed. Yet the effectiveness of the sensory component in regeneration is not diminished when its central connections are broken, as has been shown in many experiments (Locatelli, 1924, 1925, 1929; Singer, 1943; Sidman and Singer, 1951; and others) in which the motor root was destroyed and the sensory one transected between the spinal cord and the ganglion. In this way the sensory supply of the limb was separated from its connections with the central nervous system without the destruction of the sensory cell body and its fiber connections with the periphery. Therefore impulses having their origin within the central nervous system and travelling in a reverse direction out along the sensory axis cannot account for the activity of the sensory component in peripheral regeneration. The results of isolation of the sensory neuron also show that reflex connections of neurons which are so typical a phenomenon of impulse conduction are not essential in the activity of the nerve in regeneration (Sidman and Singer, 1951). This fact constitutes another important reason for separating trophic activity of nerves and impulse conduction.

A possibility which must not be overlooked before one rejects impulse flow as the mechanism of nerve action in regeneration is that impulses resembling those that ordinarily flow in neurons may arise spontaneously within the neuron even when it is isolated from other connections. Such spontaneous activity of individual neurons and their possible trophic effects have yet to be demonstrated, although it is known that an isolated neuronal system, for example, a segment of the spinal cord, is capable of individual spontaneous activity (Weiss, 1941). In this connection the experiments of Frazee (1909), who described an increased rate of regeneration in the tadpole and salamander resulting from electrical stimulation, may be worth further evaluation.

One may think of the nervous activity in regeneration as being an impulse of another sort, such as Parker (1932, 1935) has conceived in his neurohumoral hypothesis. Humoral substances produced in the cell body would radiate thence along the fibers irrespective of other polar phenomena and be impressed upon the periphery. In this way chemical substances may be thought of as percolating along the fibers in all directions from the cell body and be responsible for distal "trophic" phenomena.

THE MEDIATION OF THE NERVE INFLUENCE

The nature of the neuronal contribution to the regeneration process is thus not known. The idea has been advanced a number of times that a chemical emanation from nerve fibers may be the mechanism mediating the nerve's influence in regeneration (Schotté and Butler, 1941a; Singer, 1943; Karczmar, 1946). Schotté and Butler (1941a) expressed the belief that this chemical contribution influences a delicate equilibrium which exists in the amputation surface of the limb and in the forming blastema (see also Butler and Schotté, 1949). Considering the substantial information available in the literature on chemical mediation of the nerve impulse, the possibility that chemical substances coming from the nerve are the agents of the nerve's effect on regeneration seems reasonable. The nervous impulse is now known to be mediated at some synapses or other endings by chemical substances which transmit it to the second structure. Indeed, it is believed by some that events involving the chemical mediator are responsible for the electrically-measurable impulses along the axis cylinder. In the case of most sympathetic postganglionic endings the chemical

mediator is believed to be related to epinephrine, and for other endings acetylcholine is the agent of neuronal expression. In the case of mediation of the nerve expression in regeneration it may well be that other substances than these are given off by neurons and may effect the growth, although it is important to note that Welsh (1946) was able to stimulate regeneration in flatworms with acetylcholine. Since the nerve effect in regeneration appears not to be directly related to the conduction of impulses, it may well be that substances other than those concerned with impulses are involved in regeneration.

If the effect is chemical, then one may envision the blastema and the tissues near the cut surface as a chemical bath in which the substance or substances are poured from the nerve. Threshold phenomena as reported in preceding pages may now be described in terms of the quantity of the chemical mediator which the fibers give off. Each fiber, according to its own capacity, would pour the substance into the distal wound and the area of the forming blastema. If the sum of the concentration of the individual contributions satisfies a threshold value, then the nerve activity would receive expression in a response from surrounding tissues (Singer, 1949). The threshold demands of the responding tissue must be met continuously by neuronal activity, for should they not be satisfied, as after extensive denervation, then regenerative activity would cease or be profoundly affected. Thus, a quantity of fibers below threshold needs would not keep the concentration high enough in the blastema, even though the intact fibers were actively excreting the chemical substance. The necessity for maintenance of an adequate concentration in the blastema would be especially important if the substance is labile.

Certain findings already reported seem to sustain the idea that a chemical emanation of the nerve fiber may be the mediator of nerve action in regeneration. Extracts of the central nervous system are highly stimulatory to cells in tissue culture (Hoffman, 1940; and others); and von Muralt (1946) has described an extractable factor of brain which speeds up nerve regeneration.

OTHER PHENOMENA OF NERVE ACTIVITY WHICH RESEMBLE THE ACTION OF THE NERVE IN REGENERATION

The influence of the nerve in the regeneration of the extremities of amphibians is not an isolated neural phenomenon. Growth-stimulating and growth-maintaining powers of the nervous system are to be observed in other situations, and it will be instructive in concluding this review to point out a few examples of such activities. It is now well known through the works of Parker (1932) and his students (Olmsted, 1920a, b, 1921; May, 1925; Torrey, 1934, 1940; and others) that the vertebrate taste bud does not maintain its morphology for long if the sensory fibers of supply are interrupted (compare Stone, 1940). The receptors degenerate and finally disappear; but when the nerve fibers grow back to the oral epithelium, the taste buds differentiate once again. This effect of the nerve has been studied especially in the fish, where the taste buds present on barbels or in the skin over the body are readily available for experimentation.

The fact that sensory fibers are active in maintaining taste buds, as they also are in limb regeneration, reemphasizes the probable independence of the conduction of impulses and such "trophic" activity of the nerve (Parker, 1932). The nerve impulse runs along these fibers from the receptor to gustatory centers of the brain; but the trophic action of the nerve is centrifugal, from the neuron to the receptor.

Another example of the importance of trophic action of the nerve in the maintenance of the form as well as function of peripheral structures is to be observed in the lateral line system of fish (Brockelbank, 1925; Parker, 1932; Bailey, 1937), although it has been denied for amphibians by Stone (1933, 1937) and Speidel (1947). The lateral line receptors of the fish also degenerate upon loss of the cranial nerve of supply. Those receptors closest to the level of nerve interruption degenerate first, followed in order by those located more distally. The fact that the receptors degenerate progressively, beginning with the most rostral ones, has been interpreted by Parker (1932) to mean that the trophic impulse, so called, flows down the axis cylinder at a slow but measurable rate and thence to the receptor. When the fiber is interrupted, the source is also cut off; but the humoral substance within the distal stump continues its movement to the periphery until it is exhausted. It is exhausted first in the shortest fibers, namely, those supplying the nearest receptors, and last in the longest fibers, which go to the most distant receptors. By recording the time of degeneration of the receptors when transection of the nerve was made at different body levels, Parker was able to

measure the speed of conduction of the trophic impulse.

These examples of nerve influence on taste buds and lateral line sense organs by no means exhaust reported effects of nerve in the growth and maintenance of peripheral structures. Some others are the effects on muscle not attributable to paralysis (Tower, 1935, 1937), on mitosis in the epidermis (Overton, 1950), and the maintaining effect of one neuron upon another (Bodian, 1942; Hamburger and Levi-Montalcini, 1950). Examples of the latter are the well known transneuronal (or transsynaptic) degeneration within the lateral geniculate body after transection of the optic tract; and the gradual degeneration of nuclear centers within the thalamus after destruction of their afferent tracts. Finally, the fact that the metabolism of adipose tissue suffers certain changes when the innervating nerves are cut (Wells, 1940) needs also to be noted here.

All this information stresses the importance of the nerve in activities other than conduction of impulses. The neuron is a living cell which is, indeed, specialized for impulse conduction but, as a dynamic unit, is also capable of exerting other important effects on the structures innervated. These effects are often elusive since they are covered over by the more dramatic function of impulse conduction (compare Hamburger and Levi-Montalcini, 1950). However, once uncovered, they will undoubtedly be shown to be of great consequence for the well-being of other tissues and of profound importance commensurate with the widespread pervasion of tissues by the nerves.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionally appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Associate Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

THE RISE OF SCIENTIFIC PHILOSOPHY. *By Hans Reichenbach. University of California Press, Berkeley and Los Angeles.* $3.75. xii + 333 pp. 1951.

Giving both an historical and systematic analysis, Reichenbach presents a workmanlike introduction to the philosophy of science. Following the traditional dialectic of the schools, he considers, and rejects, the rationalist, naive empiricist, and critical (Kantian) approaches to the problems involved.

The rationalist, concerned with the contradictions of experience, resorts to mathematics and logic. Unfortunately, there is no criterion to indicate which formal system, if indeed there is any, can explain the external world. The empiricist, beginning with immediate sensation, and denying causality for the lack of any sense data to support it, can only speak with assurance of the past and present, never of the future. The introduction of the synthetic a priori by Kant is, to Reichenbach, the last supreme attempt of the rationalist to superimpose natural law by means of a logic free from internal contradictions. But synthetic propositions can be verified only by observation and hence cannot be a priori. That propositions are declared true on the grounds that they are necessary presuppositions of experience is an invalid procedure, and Reichenbach points to Kant's Achilles' heel, the replacement of Euclidean geometry by non-Euclidean, and the fall of Aristotelian logic after 20 centuries of acceptance.

It remains, therefore, for the empiricist to predict the future without any a priori prejudices concerning causality, and with a logic consisting of analytic (tautological) propositions. The frequency theory of probability is assigned this role.

This book obviously was designed for the student of philosophy, and not for the scientist concerned with those critical areas in his field that merge with philosophy.

MARK BLANK

THE WORKS OF GEORGE BERKELEY, BISHOP OF CLOYNE. *Vol. III. Alciphron, or the Minute Philosopher. Edited by T. E. Jessop. Thomas Nelson & Sons, London, Edinburgh, Paris, Melbourne, Toronto, and New York.* 30s. vi + 337 pp. 1950.

This, the longest of Berkeley's works, although his literary masterpiece, is little read nowadays. For biologists,

it does not have the particular interest of his *Essay Towards a New Theory of Vision* or the *Treatise Concerning the Principles of Human Knowledge*. Yet scientists, among whom there are free thinkers, may find a worthy opponent of such views in that master of clarity and logic, the Irish bishop, George Berkeley.

BIOLOGY: HISTORY AND BIOGRAPHY

LEBENSERINNERUNGEN. *Aufzeichnungen eines Forschers und Denkers in entscheidender Zeit.*

By Hans Driesch. Ernst Reinhardt Verlag, Basel. S. Fr. 11.00 (paper). 311 pp. + 4 pl. 1951.

Hans Driesch's *Lebenserinnerungen*, published 10 years after his death, provide a volume that should prove of considerable interest to embryologists, philosophers, travelers, and men of general good will. Driesch himself flourished in all of these capacities. The present generation of American biologists is familiar with him principally as one of the founders of experimental embryology who separated the blastomeres of the echinoderm egg and who was driven by the resultant whole embryos out of biology into vitalism and philosophy. It it his due that we know more of him. He roamed far and wide, both geographically and intellectually, and his reminiscences furnish a welcome source of information about a thinker and an individual who deserves more intimate recognition than he has been accorded by many who speak glibly of his historical position with reference to embryology.

His scientific interests included mathematics and theoretical physics as well as zoology. On the philosophical side, his original interest in the vitalistic aspects of theoretical biology led him into both logic and metaphysics. Theories of knowledge, the limits between science and philosophy, the relationships between causality and teleology and between vitalism and history all occupied his attention. He was drawn, too, from more abstract considerations towards human problems of freedom and individuality, and throughout most of his life (though he became understandably disillusioned at the end) his double faith in the power of reason and in the soul of man was a guiding conviction which pervaded all of his thinking and which fused it whole. His book on ethics, *Die sittliche Tat. Ein moralphilosophisches Versuch*, published on his 60th birthday in 1927, was banned by the Nazis in 1941.

He was fortunate to have independent means, which before the Nazi catastrophe allowed him freedom of movement as well as of thought, and he was an inveterate and a sensitive traveler. Western and Central Europe, India, Russia, the Orient, South America, the United States all figured in his itineraries, and he has written of his experiences as a voyager with both gaiety and perception.

An exceptionally genial and congenial personality shines through all of his observations on his personal and intellectual life. If he had a great fault, it was his gullibility—exemplified for instance by his advocacy of parapsychology (he was, in fact, President for a time of the London Society for Psychic Research), at a time when this discipline was pursued on a different basis and was considered less rigorous and less respectable than in some quarters today—but his credulity was that of an innocent and honest man. His character is perhaps best illuminated by his remarks about Wilhelm Roux: "Wilhelm Roux gehört in die allerbeste Reihe der Biologen aller Zeiten; er ist nur von wenigen Zeitgenossen in seiner überragenden Bedeutung gewürdigt worden. Leicht lesen sich seine Schriften nicht, aber es ist noch heute stets ein grosser geistiger Genuss für mich, wenn ich eine seiner Schriften wieder einmal vornehme. Französischer Abstammung war er, der Sohn eines berühmten Fechtmeisters; er verpflanzte die Kunst des Vaters auf das geistige Gebiet und brachte sie hier zur höchste Vollendung. Das oft missbrauchte Wort 'klassisch' ist hier am Platz." He enjoyed the happiest of relationships with many notable contemporaries in various fields, and he has succeeded in recreating his experiences with great humility and simplicity and with singular charm. This book is therefore recommended as thoroughly delightful reading for any who may wish to indulge in a particularly rewarding literary experience.

JANE OPPENHEIMER

PERSÖNLICHKEIT UND BEDEUTUNG FÜR BIOLOGIE UND PHILOSOPHIE VON HEUTE.

By Hans Driesch; contributions by Margarete Driesch, G. v. Natzmer, Ulrich Schöndorfer, Aloys Wenzl, and, A. Mittasch. Ernst Reinhardt Verlag AG., Basel, Switzerland. S.Fr. 11.00 (paper); 13.50 (cloth) 221 pp. + 1 pl. 1951.

This beautifully printed book, which is a sort of companion volume to Driesch's posthumously published *Lebenserinnerungen*, reviewed above, comprises principally a group of essays concerning Driesch's significance for contemporary philosophy. It begins with a short biography of Driesch by his wife; it includes a short discussion of his contribution to theoretical biology by Gert von Natzmer, and two more general analyses of his philosophical work: a shorter one by Ulrich Schöndorfer which surveys the development of his whole philosophical Anschauung, and a longer one by the editor, occupying more than half the book, which analyzes in considerable detail the position of Driesch as a neovitalist in relationship to the status of this field today. The volume concludes with an exchange of letters between Driesch and Alwin Mittasch, most of them written by Driesch between 1935 and 1941, and with a bibliography of Driesch's writings, including 289 titles.

The essays are each one of them original, thoughtful, and interesting. Analytical though they are, however, they all tend to be rather adulatory than critical in tone. Driesch had a great enthusiasm and an agile mind, but it does not necessarily detract from his breadth of knowledge or his fertility of ideas to admit that he was sometimes naive; whether he can with justice be named "einer der wenigen universalen Denker unserer Zeit" or "ein eigentlicher Schöpfer der modernen theoretischen Biologie" remains open to question. Be that as it may, these essays will serve as a welcome commentary on the writings of Driesch to those who are familiar with them and as an excellent introduction to his thought for those who are not yet so fortunate as to know his works at firsthand.

It is no disparagement to the present volume to wish that it had been possible to reproduce in it the obituary of Driesch, by his good friend Curt Herbst, which appeared in *Roux' Archiv* in 1941, which takes up in greater detail and from a different aspect than von Natzmer's critique Driesch's experimental accomplishments, and which remains for all time the truly definitive analysis of his contribution to experimental and theoretical biology. "Fassen wir schliesslich noch einmal Hans Drieschs Lebenswerk kurz zusammen," concluded Herbst, "so können wir sagen: Er hat in seiner Jugend durch einfache, genial erdachte Experimente der biologischen Forschung neue Bahnen eröffnet und durch scharfsinnige Deutung der Resultate derselben unsere Naturerkenntnis bis an die letzten Grenzen menschlichen Erkenntnisvermögens erweitert, wo es wohl noch weitere Fragen, aber keine gesicherten Antworten mehr gibt. Er hat unsern Besitz an geistigen Gütern, welcher der einzig bleibende eines Volkes ist, gewaltig vermehrt. Wir danken ihm dafür."

JANE OPPENHEIMER

MEDICAL CARE FOR SEAMEN. *The Origin of Public Medical Service in the United States. Vol. I.* *By Robert Straus, with a preface by Leonard A. Scheele. Yale University Press, New Haven, Conn.; Geoffrey Cumberlege, Oxford University Press, London.* $3.75. xvi + 165 pp. 1950.

This book should be of especial interest to sociologists and to persons concerned with public health and medical care. It relates the conditions under which the federal government entered the field of medical services, the needs of such services, the dangers, particularly of locally administered programs; and it suggests the possible future trends in public medical care. That the subject has been approached chiefly through an examination of the development of medical care for the American merchant seamen is understandable, since they were the first group for which such federal service was provided, and the United States Public Health Service is a direct outgrowth of it. This book is an exceptionally interestingly written book on a subject that could easily lend itself to a treatise dry as dust.

DAVID B. TYLER

A HISTORY OF ENGLISH PUBLIC HEALTH 1834–1939. *By W. M. Frazer. Bailliere, Tindall and Cox, London; [Williams & Wilkins Co., Baltimore].* 35s. xiv + 498 pp. + 16 pl. 1950.

Since the middle of the last century, especially since the Public Health Act of 1848, the British have been leading in the field of Public Health. A survey of one hundred years of their efforts and accomplishments is most welcome, all the more as a good general history of this field of medicine, in its effects most important though least noticed, is still missing, the unforgettable *English Sanitary Institutions* of that great public health pioneer Sir John Simon covering only about one-third of the "golden century" of Public Health.

Frazer deals first with a period which was essentially closed around 1900 and which he calls the "period of environmental hygiene." This period did away with those acute infectious diseases which could be controlled by changes in sewerage and water supply. In order to control tuberculosis, infant mortality, venereal diseases, etc., other methods were needed. These characterized a second period of "personal health and social services." The continuous sponsorship of research by public health authorities is of great interest. The author closes his history appropriately in 1939, before the inauguration of the great new changes in the British medical set-up.

The author is a competent guide through the sometimes complicated developments of British public health. He gives interesting economic, demographic, and epidemiological data throughout his text. He tries to integrate his story with the general history of medicine. As a writer he is more solid than brilliant, a shortcoming largely compensated by the intrinsic interest of his subject matter. The illustrations are good.

ERWIN H. ACKERKNECHT

FREUD. *His life and His Mind. New Reprint Edition. By Helen Walker Puner. Grosset & Dunlap, New York.* $1.49. 256 pp. 1950.

This book, apparently an American edition of a book previously published in Great Britain, is an extraordinarily interesting biography of Sigmund Freud. It attempts to show how those significant experiences in his life, by which our modern culture has been enriched, go back to the emotional components active in his earliest life situation and directed toward his mother, father, and siblings. Time and again experiences occurred in his life which to the author seem to represent repetitions of the forces at work in his earliest situation,

and for which he never found any satisfying resolutions. She makes a considerable issue of his ambivalent attitude toward being a Jew, and how this attitude involved his decision to cast upon Jung the leadership of international psychoanalytic movement for a brief time; and she sees in his final book, *Moses and Monotheism* again the final harassing conclusion of this ambivalent struggle. Most poignant of all is the author's demonstration of the application in Freud's case of the famous injunction, "Physician, heal thyself," and its failure. He starts out a lonely figure. He ends up a lonely figure despite her acclaim.

The book is full of sidelights on Freud's relationships with many people: his family, professional colleagues, friends. It ties together a great many isolated facts which have been gleaned from his writings and which appear never to have been put together before.

The chapter on certain modern developments or deviants from a strictly orthodox Freudian viewpoint which have sprung up in recent years, and at least one of which carries the author's implied blessing, is a chapter which has no relevance for the principal subject of the book but throws additional light on the attitude of the author herself toward her subject. This I gather is one of profound admiration, tempered with criticism for Freud's possessiveness of his discoveries and a certain irksomeness over his narrow, parochial point of view. It would appear that Freud, despite his magnificent capacity for the dissection of the human psychology, was not a true physician in the sense of living essentially on the warmth generated by the gratitude of patients and the feeling of helping humanity. He was coldly scientific, and his turning away with some spirit of pessimism concerning the value of psychoanalysis as a therapeutic tool to pursue its application in other fields, at that time so untouched, is certainly the spirit of the pioneer and explorer.

The book is very well written indeed—certainly it is the most absorbing account of Freud's life and works that I have encountered to date. Without any knowledge of who Helen Walker Puner is, it is only possible to express considerable admiration for her grasp of the subject.

WENDELL MUNCIE

ADLER'S PLACE IN PSYCHOLOGY.

By Lewis Way; introduction by Alexandra Adler. George Allen & Unwin, London; [Macmillan Co., New York]. $4.50. 334 pp. 1950.

In a brief introduction, Alexandra Adler states that the author "has been able to expound and explain Individual Psychology from a more highly integrated viewpoint." One can agree indeed with this summing up of the book. The reader will find in the first 9 chapters a clear and precise presentation of the principal formulations of Individual Psychology (inferiority and its compensations; wholeness and purpose; the guiding fiction; the neurotic character), as well as an excellent treatment of special items (sex and its deviations; anxiety and neurosis; the nature of social interest; problems of practical adaptation and the prevention and cure of neurosis). Throughout these chapters the author traces the origin and development of Adler's main concepts and compares them with other preceding and contemporary psychological systems. The three concluding chapters—a critique of Psychoanalysis; Individual Psychology and rival schools; Individual Psychology and social ideals—are more specifically devoted to the task of finding Adler's actual place in psychology. They are written with knowledge, critical balance, and with so much fairness and sense of proportion that even followers of rival schools may read them with pleasure. The author points out that one major criticism against Individual Psychology seems justified. "It is the opinion of the present writer . . . that a bias towards the extraverted view does show itself throughout Adler's work, and that the problem of man's relationship to his community, true insofar as it goes, is incompletely stated in his social interest. . . . It is possible to argue that this therapy shows certain tendencies to brush aside problems of real consequence to the individual."

WALTER O. JAHRREISS

SCIENCE AND RATIONALISM IN THE GOVERNMENT OF LOUIS XIV, 1661–1683. *The Johns Hopkins University Studies in Historical and Political Science, Series LXVI, number 2.*

By James E. King. The Johns Hopkins Press, Baltimore. $3.75. 337 pp. 1949.

While of considerable interest to students of science in general, and to those of the influence of Cartesianism in particular, this scholarly study has in it little if any reference to the biological thinking of the day.

THE YOUNG NATURALIST

THE ADVENTURES OF MAYA THE BEE.

By Waldemar Bonsels; translated by Adele Szold Seltzer; illustrations by Rafaello Busoni. Pellegrini & Cudahy, New York. $3.00. 191 pp.; ill. 1951.

The American Military Government has done many things in its varied career, but one of its nicest and probably least publicized acts was to reprint for its overseas families, and thus to repopularize, the old and wonderful story of Maya the Bee. The present edition stems from this renewed popularity. A fanciful and delightful tale of a bee who must see the world for herself, the story is a German classic which has passed into many languages since first published nearly fifty

years ago. Although written for children, its timelessness and charm will be evident to any adult who picks it up for an evening's reading.

C. P. SWANSON

MOTHER NATURE'S CHILDREN.

By Mary I. Curtis; pictures by Julia Morrison. Lyons and Carnahan, Chicago. 96 cents. 80 pp.; ill. 1950.

This is a third grader's book of the rain, the wind, and the sun, and of plants and animals as they are affected by the elements. Through imaginary conversations with each other, the role of each in nature's economy and extravagance is opened to the child in a sprightly and informative way.

ECOLOGY AND NATURAL HISTORY

LESSONS ON SOIL. *2nd Edition.*

By Sir E. J. Russell. Cambridge University Press, New York, and London. $1.50. xx + 134 pp.; ill. 1950.

Although reprinted frequently, the first edition of this book appeared in 1911. A second edition is of course warranted on the basis of much that has been learned since that time, but also because of the greater need for a reemphasis upon good soil conservation and utilization in the light of Britain's austerity program and increasing population. The book is actually a series of lecture-experiment combinations on soils, plant food, soil inhabitants, management, type distributions, and soil origins. The audience addressed is what would correspond in America to the junior high school level, and the objective is to create an understanding of the soils which contribute so much to the well-being and productivity of a nation. This is a readable and informative textbook, with many experiments suggested for the young student.

LONDON'S EPPING FOREST.

By James A. Brimble. Country Life, Covent Garden, England. 30s. 176 pp. 1950.

Epping Forest, the former hunting ground of the English royalty, is now the area to which Londoners may journey to picnic and view what remains of a once vast forest area. J. A. Brimble's book is a history of the forest, and a descriptive appreciation of its beauties and its traditions. As such, it is largely of interest to those familiar with this region of England, yet no one can fail to appreciate the beauty of the area, conveyed through the numerous, superb photographs.

C. P. SWANSON

PLANT AND ANIMAL GEOGRAPHY *3rd Edition.*

By Marion I. Newbigin. E. P. Dutton & Co., New York; Methuen & Co., London. $5.00. xvi + 298 pp.; ill. 1950.

This biogeography is in no sense modern. It depends for its basic data largely upon Engler & Drude (1896–), Schimper (1903–1935), Warming (1909, 1930), Tansley (1911–39), Hardy (1920), de Martonne (1927), Supan (1930), Braun-Blanquet (1932), and Gaussen (1933). There are only about a dozen other, lesser references. These books are giants, of course, but one looks in vain for any evidence of modern biogeography in the form to be found in a now copious literature in journals and monographs and in several books on the subject published in advance of this so-called third edition. No criticism can be implied of the late author, nor of Florence M. Newbigin, who prepared the original text from Marion I. Newbigin's unfinished manuscript, but it seems to me that the publisher has a responsibility to the interested public and should not label as a new edition a book essentially unchanged since its critical notices some 15 years ago.

STANLEY A. CAIN

OF SOCIETIES AND MEN.

By Caryl P. Haskins. W. W. Norton & Co., New York. $4.50. xiv + 282 pp. 1951.

Many attempts have been made in the past to use observations on social insects and other animal societies as a basis for conclusions concerning the social behavior of man. With rare exceptions, such as Kropotkin's *Mutual Aid*, the results have been deplorable. It is the more gratifying to find that the book by C. P. Haskins is a serious and important contribution to the question whether something useful for the future of mankind may be gleaned from the ancient evolutionary history of animal societies. The author stresses the existence of three important parameters of social evolution. "The first is a trend [well-nigh universal for the evolution of all life] toward the increasing complexity of societies with the passage of evolutionary time. . . . The second is more limited. It involves a concurrent trend toward the more efficient integration of social structures. . . . The third parameter is a tendency, under certain special conditions and at certain social levels, toward a reversal of the trend to complexity leading back along the road of modification or loss of role and function to greater over-all simplicity and sharper specialization." These parameters are as important in the evolution of societies as in that of solitary species. In primitive organisms lack of parental care generally is made up by high fecundity; in more highly organized creatures greater solicitude for the young limits fecundity; but life in a social community allows the combination of protection and high fecundity, obviously a great advantage.

The associative form of animal societies (herds and

flocks) in which there is less pronounced integration, specialization, and secondary simplification is of particular interest for man. Important similarities to the foundations of our own social life relate, inter alia, to the herd instinct (fear of solitude, rewards of communication), physiological family bonds, and advantages of specialization, but the forces of cohesion are very different. "Because human and non-human societies alike show the impress of the trends to complexity and to specialization and hence face similar evolutionary dangers, it has too often been assumed that the trend to integration affects them in similar ways, and that if human societies are to survive they must go the way of the insect colony in evolution, exhibiting tighter and tighter integration, with correspondingly less and less of individual antonomy." But in man the associative structure is much more specialized than the family pattern and "continues to evolve dynamically at a rate which today far outstrips the social development of any organism that earth has known." The opposite trends and opposing loyalties of family ties and demands from associative society are creating stresses which must be bridged and harmonized. By his intellectual capacities man has in his social existence to a large extent escaped adaptive specialization, thereby preserving more adaptability, and providing at once for the possibility of biological and cultural evolution, the latter creating integration by transmission of knowledge and traditions. But Haskins emphasizes rightly that "our instinct endowment may be more important than we usually think." Man's intellect gives him the immense power "of *choosing the direction of his own social evolution*." We find ourselves now in the midst of a life-and-death struggle for this very choice. The ever-growing complexity of human society has led to a drive to integration and dependence of the individual upon the whole, an integration that is fundamentally foreign to the associative nature of man. These trends have brought violent contradictions and strains into man's institutions and into his existence as an individual. The temptation has become great (especially under pressure of economic forces, not discussed by Haskins) to yield in the direction of least resistance by surrendering to the extremes of integration (totalitarianism). To struggle for a satisfying balance between the opposing forces of individual freedom, associative desires, and integrative trends will presumably be the continuing lot of man.

Such is an incomplete and imperfect sketch of the author's general argument. There is no room here to deal with the wealth of exciting detail drawn from a vast literature on animal societies, nor is there need to quibble over this or that subsidiary assumption with which not everyone may agree. The main conclusions are clear enough—and compelling. There is nothing in this book of the dreamer's paradise in which it is believed that the ethical principles of Christianity have "tended toward the further evolution of mankind and that they will continue to do so in the foreseeable future" (Waddington). There is nothing here of the naive, metaphysical and crudely ad hoc normative judgments common in the current literature on evolution, such as that "individuals vary greatly in other capacities, but integrity and dignity are capable of equal development in all" (Simpson). There is no claim here for such romantic and biologically indefinable postulates as that "it is ethically right to aim at whatever will promote the increasingly full realization of increasingly higher values" (Huxley). There is nothing here, in short, that could have provoked the wrathful and altogether justified outburst of Sir Arthur Keith (however much one may differ with him on his own ground) that in view of some of these sentiments he "was moved to wonder whether all science is founded on the rock of truth or on the quicksand of opinion." Haskins does not carry his arguments beyond the sphere of biology, strengthening greatly the conviction of his conclusions by this restraint. Many and vital problems remain to be studied, not the least of which perhaps is the question how man with a relatively invariable capital of mental faculties can cope with the increasing complexities of existence without losing that which distinguishes him from the integrated societies of ants and termites. Many of the final conclusions arrived at by Haskins have been carried further in a most important fashion by Jules Romain (*Le Problème Numéro Un*. Librairie Plon, Paris, 1947).

WALTER LANDAUER

ECOLOGY OF FORAMINIFERA, NORTHWEST GULF OF MEXICO. *Part I: Foraminifera Distribution. Part II: Foraminifera Species. Geological Society of America Memoir 46.*

By Fred B. Phleger (I); Fred B. Phleger and Frances L. Parker (II). Geological Society of America, New York. $2.75. Part I: x + 88 pp. + 1 table; ill. Part II: iv + 64 pp. + 20 pl. + 10 tables. 1951.

This memoir consists of 2 independently paged parts. The first, of 88 pages, dealing with the distribution of the foraminifera, is by Phleger. The second, consisting of 63 pages and 20 plates, is a systematic treatment of the species by Phleger and Parker together. Several new species are described. Of the total fauna, some 15 per cent are known only from the Gulf of Mexico. The first part presents in summary form the temperature and salinity data obtained by the *Atlantis* in the early part of 1947 in the northwest Gulf of Mexico. This information will be useful to students of other groups in the area. In the core samples a relatively cold water fauna was found, possibly of late glacial age, and suggestive of water conditions now found between Cape Hatteras and Cape Cod.

J. W. HEDGPETH

ECOLOGY OF ANIMAL PARASITES.

By Jean G. Baer. University of Illinois Press, Urbana. $5.00. xii + 224 pp.; ill. 1951.

Few biologists will question the author's view that "the varied aspects of parasitism, symbiosis, commensalism, and phoresis are as truly within the scope of ecology as are the problems of predation, succession, geographical distribution, and relations with the physical environment." The present volume is an attempt "to review the field of parasitology from all possible angles and to show that parasites are subject to the same general laws that govern all free-living organisms." The fact that parasitology has progressed to a place where it is impossible for one person to be really conversant with the whole field makes such a goal almost unattainable.

Following a short section on the different types of animal association, there are sections on adaptations to parasitism, host-parasite relationships, physiology, and the origin of parasitism. In the longest section, that on adaptations to parasitism, the author has selected a series of remarkable examples from various groups, including such infrequently considered groups as the mollusks, turbellarians, and crustaceans. These, he discusses from the standpoint of morphology, taxonomy, life cycle, evolution, and the effects of the parasitic habit. In general, he has selected material which is not treated in medical and veterinary textbooks. The resulting account is excellent and most professional parasitologists will find much that is new to them in it.

The first chapter of the section on host-parasite relationships deals largely with the taxonomic aspects of host-specificity and shows the same high degree of excellence as the first part of the book. The remainder of the section, however, deals largely with immunological aspects, broadly defined, but ignores most of the fundamental biological work in that field. In view of the fact that the interaction of host and parasite constitutes one of the unique ecological characteristics of parasitism, it is to be regretted that the author could not have given more pertinent examples and worked sounder generalizations into the picture of how a parasite survives in an environment containing various immunologically non-specific and/or specific (antibodies) antiparasitic factors. Furthermore, in a book having the term "ecology" in the title, greater attention could have been paid to modern work stressing the statistical (census) interaction between host and parasite when the two are considered, not taxonomically, but as competing populations. This point of view has been given a general but cogent hearing by a non-ecologist, F. M. Burnet, in the first chapter of his book *Biological Aspects of Infectious Disease*, and is developed in both epidemiological and ecological studies from the inductive and deductive aspects. The section on physiology is largely a limited consideration of nutrition. Detailed studies on the carbohydrate metabolism of the malarial parasite and other parasites are omitted, although they obviously have an important bearing on the origin of parasitism.

In spite of the immunological and biochemical shortcomings, the book will be of great interest to parasitologists, ecologists, and biologists because it is thoroughly biological in its concepts and contains many stimulating speculations.

W. H. TALIAFERRO

ARMY-ANT LIFE AND BEHAVIOR UNDER DRY-SEASON CONDITIONS. *4. Further Investigation of Cyclic Processes in Behavioral and Reproductive Functions. Bull. Amer. Mus. nat. Hist., Vol. 95, Art. 5.*

By T. C. Schneirla and Robert Zanes Brown. American Museum of Natural History, New York. $1.25 (paper). Pp. 263–354 + 1 pl.; text ill. 1950.

Studies of *Eciton hamatum* and *E. burchelli* colonies in Panama in the 1948 season have confirmed and extended previous work. The use of permanently marked queens promises new insight into colony functions and mobility, and duration of function on the part of single queens.

MOSQUITO BEHAVIOUR *in Relation to Malaria Transmission and Control in the Tropics.*

By R. C. Muirhead-Thomson. Edward Arnold and Company, London; Longmans, Green and Company, New York. $5.50. viii + 219 pp. + 16 pl.; text ill. 1951.

Muirhead-Thomson has carried out some beautiful studies of anopheline ecology in India and Africa; and these, placed in the frame of other recent studies of anopheline behavior, form the basis of this book. "Control" is emphasized in the title and at various places in the text, and the book will undoubtedly be useful to every malariologist; but this should not lead academic biologists to ignore the material. The text is written, really, from the point of view of the study of animal behavior, rather than from the point of view of medical entomology as we know it in the United States.

The author presupposes some acquaintance with tropical anophelines, and apparently intended his book to be an extension and development of the recent mosquito reviews to which he makes reference. But much of the material will surely be understandable in itself to the student of insect ecology. Leads that seem promising for further work are emphasized all the way through, which should make the book particularly stimulating to students working in temperate or tropical America, where studies of mosquito biology have not developed as rapidly as in the Old World.

Chapter headings are: General Activities in Relation to Mating, Feeding and Longevity; Nocturnal Activity and Biting Cycles; Outdoor Resting Places and Exodus

from Houses; Host Selection; Behavior in Relation to Temperature, Humidity and Light; Behavior in Houses in Relation to Control by Insecticides; Selection of Breeding Place; Light, Shade and Mechanical Obstruction; Water Movement and Silt; Water Temperature; Dissolved Oxygen, Organic Matter and pollution; Salt Water and Tidal influence.

MARSTON BATES

SPOTLIGHT ON ANIMALS.

By Chapman Pincher; illustrations by Eric Tansley. Hutchinson & Co., London, New York, Melbourne, Sydney, and Cape Town. 12s. 6d. 188 pp.; ill. 1950.

An entertaining book into which to dip is this product of the Scientific Correspondent of a British newspaper. He has ranged over the animal kingdom, expanding news items about current research work, or just oddities, into short essays averaging under 4 pages each. Clear scratchboard illustrations by Eric Tansley help the lively text.

LORUS J. & MARGERY J. MILNE

LIFE HISTORIES OF NORTH AMERICAN WILD FOWL. (*Order Anseres.*) *Ducks, Geese and Swans. Vols. I and II.*

By Arthur Cleveland Bent. Dover Publications, New York. $8.00 the set. Vol. I: x + 244 pp. + 46 pl. Vol. II: x + 316 pp. + 60 pl. 1951.

The first of A. C. Bent's monumental series of volumes on the life histories of North American birds appeared in 1919 and the eighteenth in 1950. The venerable author, now in his eighty-fifth year, is nearing the end of his task, although 3 more volumes will be required for its completion. The early numbers in this series, issued in small editions, have become scarce and command high prices. This is particularly true of the 2 volumes on waterfowl (ducks, geese, swans), which have been in great demand among wildfowlers and game technicians, as well as naturalists and bird students. The present volumes represent the second attempt by a commercial firm to reprint some of the earlier and rarer numbers in this series, and it is to be hoped that Dover Press can carry out their announced plan of reprinting the entire group.

A process of photographic reproduction has been employed, and the results are, by and large, satisfactory, though some of the photographs have become rather hazy. One must object to the absence of any indication that these volumes are *reprints*, no mention whatever being made of their earlier appearance as bulletins of the National Museum. As regards content, dozens of books and hundreds of articles on waterfowl have appeared in the 25 years since Bent completed his work on this group. Nevertheless, the continuing demand for this summary of the life histories of American waterfowl, a summary integrated by Bent's wide field experience, and by the numerous photographs with which it is illustrated, is sufficient tribute to its usefulness.

D. AMADON

MY FAVORITE STORIES OF THE GREAT OUTDOORS.

By Roy Chapman Andrews. The Greystone Press, New York. $2.98. xii + 404 pp. 1950.

Roy Chapman Andrews is a good picker of stories. These thirty-five, by such writers as William Beebe, MacKinlay Kantor, Marjorie Kinnan Rawlings, W. H. Hudson, Mark Twain, Jack London, Ernest Thompson Seton, John Burroughs, Stewart Edward White, Eric Knight, Zane Grey, Jean Henri Fabre, and others equally notable, have all the excitement of the good yarn mingled with the tang of the campfire and the sweet smells of the outdoors. The nature of animals is portrayed with keen insight and in all its variety. The life of the marsh quivers, the winds sough through the tall pines, the denizens of the ocean hurtle above the waves and flash through its depths. This is a grand book for a boy, and for those who have not quite forgotten that they were once boys, too.

BENTLEY GLASS

EVOLUTION

SPECIATION AND ECOLOGIC DISTRIBUTION IN AMERICAN JAYS OF THE GENUS APHELOCOMA. *Univ. Cal. Publ. Zool., Vol. 50, No. 3.*

By Frank A. Pitelka. University of California Press, Berkeley and Los Angeles. $3.00 (paper). Pp. 195–464 + 14 pl.; text ill. 1951.

This is the latest in a series of monographs of particularly interesting genera and species groups of American birds published by the University of California. Similar treatment has been accorded previously to the following genera, in whole or in part: *Lanius*, *Passerella*, *Junco*, *Eremophila*, *Limnodromus*, and *Pipilo*. The genus *Aphelocoma*, subject of the present paper, has no inclusive English name. *A. coerulescens*, the species most familiar to North American ornithologists, is commonly called the "Scrub Jay," and includes the "California" and "Florida" Jays. The other two species, *A. ultramarina* and *A. unicolor*, are primarily Mexican in distribution.

The reader is quickly impressed by the thoroughness and care with which Pitelka's study was made, contrasting thus with many recent "revisions" of avian genera and species. Nearly 5000 specimens were assembled, representing material from 26 museums and 7 private

collections. Unusual precautions were taken to insure accuracy of comparisons, including original techniques for segregating specimens by age classes, and selection of lighting for color comparisons. Five subspecies of *A. coerulescens* and one of *A. ultramarina* were studied in the field; the author did not encounter *A. unicolor* in the field until 1950, and his original observations on its habitat and behavior are thus necessarily confined to 2 long footnotes. Habitat is given more thorough attention than is usual in taxonomic papers of this nature and the discussion of each subspecies includes a section entitled Ecologic Distribution. There is an excellent summary of Distribution in Relation to Ecological and Historical Factors. The author dates the history of the genus from the Miocene, and relates its dispersal and that of the component species to the spread of certain plant assemblages.

This study of *Aphelocoma* resulted in the description of 6 new subspecies, 2 of which were published in earlier papers by Pitelka. The author nowhere indicates his criteria for subspecific separation of populations. He cites the statistical methods by which he tested populations for significant differences, but this is not equivalent to criteria for the recognition of subspecies. *Aphelocoma coerulescens californica* and *A. c. oocleptica* are recognized as distinct subspecies by Pitelka solely on the basis of larger size of the latter. Subjecting Pitelka's measurements of the largest population of *A. c. oocleptica* and the smallest of *A. c. californica* to the various statistical tests suggested by Amadon (*Condor*, 51: 250–258. 1949), it is found that these two "subspecies" would not be considered worthy of nomenclatorial recognition even under definitions considered too liberal by Amadon. The same may be said of *A. c. hypoleuca* and *A. c. cactophila*, separated by Pitelka primarily on the basis of size. All other subspecies are based on combinations of size and color characteristics, and cannot be verified from measurement tables alone.

In a review of a paper on salamanders in this same California series, Grobman (*Q.R.B.*, 26: 50. 1951) criticized the frequent mention of California localities and geographic features, many unfamiliar to eastern workers, which were not indicated on any map published with the paper. This is equally true of Pitelka's paper. Many easterners would have found the geographic interrelationships of populations easier to visualize if maps had been included to show the location of counties (much cited by Pitelka in range descriptions), mountain ranges, and the like. It is somewhat annoying to be forced to read the paper with an atlas open on the desk.

The plates consist of an excellent semi-diagrammatic portrayal in color of 6 forms of *Aphelocoma* by George Miksch Sutton, 9 habitat photographs, and 9 photographs of skins. The latter are of limited value in comparing coloration (important in this genus), but serve adequately to illustrate differences in pattern and proportion. An appendix gives complete synonymies, data on type specimens, and nomenclatorial histories of all the subspecies. Pitelka withholds judgment, pending further evidence, on Amadon's proposal (*Amer. Mus. Novit.*, 1251: 6-7. 1944) to include *Aphelocoma* (as well as the currently accepted genera *Cyanolyca* and *Cissilopha*) in the genus *Cyanocitta*.

This monograph, in spite of its comparatively minor defects, will take its place among the major taxonomic works on birds of North and Middle America, and is worthy of the attention of all vertebrate zoologists as a model of thoroughness and attention to detail.

KENNETH C. PARKES

FAUNAL LISTS AND DESCRIPTIONS OF PALEOZOIC CORALS. *Geol. Soc. Amer. Memoir 44.*

By R. S. Bassler. Geological Society of America, New York. $3.50. x + 315 pp. + 20 plates. 1950.

Geologists and paleontologists are much indebted to the author for having published several valuable catalogues of Paleozoic fossils. Now he has added a further volume with extensive lists of Paleozoic coral faunas of the world arranged in stratigraphic order. In them the names of the corals have been corrected in most instances to agree with present usage. Much information about the type localities, the type species, and synonymies, as well as useful stratigraphic particulars, are included in the lists. Since the author of course was not able to make a thorough revision of all the many coral faunas of different systems and countries, it is unavoidable that the lists contain many mistakes and shortcomings. Nevertheless, it must be readily admitted that they will prove of great value, and will be indispensable for everyone concerned with Paleozoic corals or faunas. Unfortunately, it was not possible to give a bibliography of the cited literature.

The second, smaller part of the book is devoted to descriptions of Ordovician tetracorals, which are of special importance because they represent the oldest and most primitive forms of the Anthozoa. Curiously enough, they have so far received much less attention than the later Paleozoic corals. Twelve genera belonging to the families Favistellidae and Tetradiidae are discussed. Two of them are new: *Eoflelcheria* (near to *Lichenaria*, but with Fasciculate corallum) and *Saffordophyllum* (near to *Nyctopora*, but with poorly developed and distantly spaced septa). About 40 new species are erected. The genus *Tetradium* alone includes now more than 35 species, which is perhaps an exaggerated number for these early and variable forms whose characters are not yet definitely fixed.

OTTO H. SCHINDEWOLF

PRE-TRAVERSE DEVONIAN PELECYPODS OF MICHIGAN. *Contrib. Mus. Paleontol. Univ. Mich., Vol. VII, No. 10 March 29, 1950.*

By Aurele La Rocque. University of Michigan Press, Ann Arbor. $2.00. Pp. 271–366; 19 pl. 1950.

In a total of 40 species of Devonian pelecypods from the pre-Traverse strata of Michigan, 14 new species and 3 new genera are described. The plates are exceptionally fine.

ANURANS FROM THE EARLY TERTIARY OF PATAGONIA. *Publications of the Scarritt-Patagonian Expedition, Number 37. Bull. Amer. Mus. nat. Hist., Vol. 93, Art. 2.*

By Bobb Schaeffer. American Museum of Natural History, New York. 50 cents (paper). iv + pp. 45–68 + 4 pl.; text ill. 1949.

Three new fossil species representing new genera are placed along with *Eupsophus* in the family Leptodactylidae. All are of lower Eocene or Oligocene age. The family, also known from the lower Eocene of India, probable center of origin of the family, has modern representatives in Africa and Australia as well as S. America. Leptodactylids differ from toads of the family Bufonidae in lacking Bidder's organ.

FRONTAL SINUS EVOLUTION (PARTICULARLY IN THE EQUIDAE). *Bull. Mus. comp. Zool., Harvard College, Vol. 103, No. 8.*

By Tilly Edinger. Museum of Comparative Zoology at Harvard College, Cambridge, Mass. $2.25 (paper). Pp. 409–496 + 9 pl.; text ill. 1950.

A highly important evolutionary study. It is shown that the voluminous frontal sinuses of the modern *Equus* were not present at all in early fossil horses from the Eocene to the Lower Miocene. The sinus first made its appearance in a *Merychippus* of the Middle Miocene. Its further great enlargement appears clearly to be a consequence of cerebral expansion which led to discongruities between the elements of the skull; that is, "spaces not used and therefore subject to pneumatization" resulted. The observations therefore accord well with Weidenreich's theory of sinus origin and evolution, namely, that "they have no function [but] occupy dead spaces which have resulted from disharmonious growth of the skull components."

PROMERYCOCHOERINAE, A NEW SUBFAMILY OF OREODONTS. *Contributions to the Revison of the Oreodonts (Merycoidodontidae), Number 4. Bull. Amer. Mus. nat. Hist., Vol. 93, Art. 3.*

By C. Bertrand Schultz and Charles H. Falkenbach. American Museum of Natural History, New York. $1.75 (paper). Pp. 69–198; ill. 1949.

This is a continuation of the revision of the oreodonts undertaken by the authors. The new mid-Miocene subfamily (found in the John Day & Harrison formations) is characterized by "small- to large-sized forms exhibiting a tendency towards brachcephalic skulls; a narrow supraoccipital region which is not fan shaped as in Merycochoerinae, Ticholeptinae, and Merychyniae; a small and shallow lacrimal fossa; an inflated muzzle with facial constriction; inflated bullae; brachyodont teeth; large C/ and P_1; and a posterior intermediate crest on P^3." 154 specimens of *Promerycochoerus*, 110 of *Mesoreodon*, 5 of *Promesoreodon*, and 6 of *Merycoides* are referred to the new subfamily.

GENETICS AND CYTOLOGY

ADVANCES IN GENETICS. *Volume IV.*

Edited by M. Demerec. Academic Press, New York. $7.50. ix + 343 pp.; ill. 1951.

Volume 4 of this series contains a group of solid and substantial reviews, well balanced as a reflection of the diversity of current genetic problems. Three articles deal with the genetics and breeding methods of economic crops—coffee, rice, and American cotton—each pointing out the possibilities of future progress as well as summarizing past evidence. Matthey and White, respectively, treat the cytology and cytogenetics of the vertebrates and the orthopteroid insects. The review by the former is an extension of his recent book (only the more recent references are added). White has adroitly handled the voluminous literature extant on the cytology of the Orthoptera, although his treatment of certain aspects of chromosome structure is cautious indeed. The remaining 4 reviews, all excellent, consider gene action, from different points of view. Glueckson-Waelsch and Hadorn both deal with the physiological action of genes in development, the former considering mainly those which interfere with organ systems in the mouse, while Hadorn views the action of lethal genes in *Drosophila* from the standpoint of an embryologist. Hannah approaches the problem of heterochromatin—its localization and function in *D. melanogaster*—with a caution which the subject needs, and with the implied conclusion that it is premature to attempt to answer the many problems revolving around this type of chromatin which is still inadequately characterized, both genetically and cytologically. The same note of caution, without being pessimistic, however, is encountered in Stephens' review of the role of duplications in evolution. In particular he stresses that while duplications appear to have played a role in evolution, with pseudo-alleles forming an incipient stage in this process, positive certainty is very difficult to attain, and in no one case, established.

C. P. SWANSON

PRINCIPLES OF GENETICS. *New Fourth Edition. McGraw-Hill Publications in the Botanical Sciences.*

By Edmund W. Sinnott, L. C. Dunn, and Th. Dobzhansky. McGraw-Hill Book Co., New York, Toronto, and London. $5.00. xiv + 505 pp.; ill.; 1 perforated, folded enclosure. 1950.

The increased popularity of courses in heredity now evident on the American campus is due to several factors. In its first fifty years as a science genetics has made great strides in solving both theoretical and practical problems. Students are aware of these achievements and elect courses in heredity not only as an integral part of their pre-professional training, but also because they are vitally concerned with problems such as radiation damage, disease resistance, food production, etc., and also because of interest in the current situation of biology in the Soviet Union.

Today more than ever before, it is essential that a student build for himself a solid foundation of fundamental principles and their applications. Moving toward this objective, it is especially important to select a textbook that will present the salient facts and principles clearly and completely. An indispensable feature of any such text is the inclusion of a well-balanced set of problems which will efficiently and clearly demonstrate, by means of student participation, the workings of these principles. Finally, the text should discuss the contributions of genetics towards an understanding of development and evolution.

The recently published fourth edition of *Principles of Genetics* by Sinnott, Dunn, and Dobzhansky, is a thorough, modern, factually up-to-date, and well written book. Chief additions occur in sections dealing with cytogenetics, population genetics, race and species formation, and a new chapter on the nature of genic action and structure. The conscientious instructor will doubtless find, as we have, that these areas evoke much student interest. There are almost 100 more pages of text in this edition than in the previous one. Because there is so much additional information, it may not be possible to cover all the material in each chapter in one semester, even in a course with a laboratory for upper classmen. Also, the statistical portions of the chapter on The Multiple-factor Hypothesis seem much too difficult for the average student. Nevertheless, this textbook has been used successfully by both of us, even in a short, thirty-hour lecture course for students without special training in biology. Beyond question, this is the best available textbook in the field of general genetics.

S. Fogel & I. H. Herskowitz

PROBLÈMES D'HÉRÉDITÉ.

By Jules Carles. Beauchesne et Ses Fils, Paris. 180 fr. (paper). 258 pp. + 2 pl.; text ill. 1945.

In the first section of this interesting treatise, the author reviews the mechanics of heredity with attention to historical aspects. Perhaps for the purpose of brevity, accounts of four-strand crossing-over and of partial sex-linkage are omitted. The second section of the book considers human heredity, hybridization, consanguineous marriages in human populations, and the application of the knowledge of blood groups to cases of questionable paternity. Each subject is discussed in admirable detail in view of the limited length of the volume as a whole. The mention of a percentage of 89 for group AB among natives of French Guinea suggests an association of blood group genes with balanced lethals in this population. In the chapter on hybridization and heterosis, it would have been fitting to have included a reference to the commercial use of hybrid corn in the U. S. A.

The last section of *Problèmes d' Hérédité* is entitled Heredity and Progress. In it the author discusses artificial and natural selection; evolution, the inheritance of acquired characters, and eugenics. In describing the production of mutations by x-rays and radium, the author should certainly have included the name of Muller with the other two investigators mentioned. Carles feels that the inheritance of acquired characters is still an open question in some cases. On philosophical grounds, he thinks some form of Lamarckism may yet prove to be true. The reviewer feels more in accord with his discussion of the positive and negative aspects of eugenics. It is reassuring to read such a scientifically moral treatment of negative eugenics written in a country where Nazi views might be expected to have had some influence. The reviewer recommends this volume to American readers not only for the material interesting to the student of heredity but also for its charming literary style.

Sarah B. Pipkin

GENERAL AND SYSTEMATIC BOTANY

BOTANY. *An Evolutionary Approach.*

By R. Darnley Gibbs. The Blakiston Co., Philadelphia and Toronto. $6.00. xiv + 554 pp.; ill. 1950.

The many botanical textbooks available today are, in the main, distinguishable one from the other by differences in emphasis and approach rather than by differences in content. The present volume, however, differs in content as well from the more recent textbooks in that the physiological aspects of growth and development receive but brief mention in comparison to the overwhelming amount of taxonomic and morphological detail included. The book, therefore, is essentially a survey of the plant kingdom. The author has chosen to call his approach an "evolutionary" one, but lest the reader be misled by the title, it should be emphasized that the term is employed in a strictly phylogenetic sense. Evolutionary concepts are divorced from cause and effect relationships until the final chapter, where evolution as a dynamic force is most sketchily presented. Hitched in this fashion to the main body of the book as a loose,

and almost useless, appendage, this chapter gives the uninitiated reader but a vague impression of the forces which operate to produce the diversity of plant forms around him. As regards evolution, the tempo and tenor of the book is adequately, if somewhat inadvertently, summed up by the author in a sentence which states: "Observation would seem to suggest that evolution at the present time, *if it occurs at all* [reviewer's italics], is on the whole rather slow."

For a course taught in the classical tradition, the book is admirably suited. It is well written and beautifully illustrated. It is not recommended for any course in which the dynamics of living things is a prime consideration.

C. P. SWANSON

THE BRITISH SMUT FUNGI (*Ustilaginales*).
By G. C. Ainsworth and Kathleen Sampson. Commonwealth Mycological Institute, Kew, Surrey, England. $3.00. 137 pp. + 2 pl.; text ill. 1950.

Prior to the appearance of this book, the most recent general account of the British smuts was that by Plowright in his *Monograph of the British Uredineae and Ustilagineae*, 1889. In the 61 years that have intervened, a great deal has been learned about the life histories and host relations of the smuts, and a great many new species and some new genera have been recognized in the world as a whole, including several in the British Isles. The present book includes these additional British species, both those that are new and those whose distribution has been extended from foreign shores. The nomenclature is brought into accordance with the latest rules. In the matter of specific segregation a conservative attitude has been maintained. The smuts are considered under the following headings: Biology; Cytology; Genetics; Technique of Study; and Classification. These are followed by the main portion of the book, a detailed study of the genera and species occurring in Britain. Thirteen of the 33 genera recognized by the authors are found in the British Isles, including 32 species in 5 genera of the Ustilaginaceae, 41 species in 8 genera of the Tilletiaceae, and a single genus with 1 species in the doubtfully related family Graphiolaceae. Each species, so far as the data are known, is discussed as to synonymy, description of the fungus, hosts, distribution, spore germination, incompatibility factors, racial specialization, and manner of infection of the host. The plates, from photographs, show the main habits of the smuts. The line figures illustrate the spores, mycelium, germination of spores, etc., of nearly all the British genera. A bibliography of about 600 references occupies over 18 pages. A combined host and pathogen index completes the book, which is of outstanding merit and deserves great praise.

ERNST A. BESSEY

KLEINE KRYPTOGAMENFLORA VON MITTELEUROPA. *Band I: Die Moos- und Farnpflanzen (Archegoniaten).*
By Prof. Dr. Helmut Gams. Gustav Fischer, Jena. DM 12.00. iv + 186 pp.; ill. 1950.

This pocket-sized book provides an effective field manual for the identification of the bryophytes and pteridophytes of Austria, Germany, and Switzerland. The author, a distinguished specialist on plant distribution, has compressed a remarkably large amount of accurate and well presented information into a very small space. The combination of excellent treatment and convenient size have caused a phenomenal demand for the book—few other purely systematic works on cryptogamic plants have required 3 editions in 10 years, especially during the war and post-war years. Several keys are presented for mosses and hepatics, among which 2 are of special interest here, one to the chief groups—genera, families, and orders—of bryophytes based primarily on the characters of the sporophyte, and the other based on gametophytic characters. The pterodophytes are treated in 2 keys, one to the families and to occasional genera, the other to all the species. In reality, the whole work consists of keys, and the descriptions of the species, which are models of brevity, are included within the framework of an extended key. The author attains real economy of space by the important technique of omitting in the descriptions that information already given earlier in the keys, yet essential facts and accuracy are not sacrificed. The text-figures, although small, are uniformly excellent, and of outstanding usefulness for field identifications because of their realistic representation of the characteristic growth habits of different species. Geographical distribution is given in general terms or precisely, if important. Adequate nomenclatorial data are included, especially the synonymy where needed. One soon comes to the realization that pocket-sized books of this sort, in English and dealing with American plants, are very greatly needed, and would find a wide demand.

WILLIAM C. STEERE

HEPATICAE OF NORTH AMERICA. *Part V. University of Washington Publications in Biology. Volume 6, Number 5, December, 1947.*
By T. C. Frye and Lois Clark. University of Washington Press, Seattle. $4.50 (paper). iv + pp. 735–1022; ill. 1947.

FLORA OF THE CAPE PENINSULA.
Edited by R. S. Adamson and T. M. Salter. Juta & Co., Cape Town and Johannesburg. £33s. 0d.; $8.82. xx + 889 pp. 1950.

Although the Cape Peninsula has been occupied longer than any other part of S. Africa and its flora has been collected more intensively than that of other areas of the Union, the exceedingly rich "Flora Capensis" has "never been described as a unit." The *Flora of the Cape Peninsula* fills this conspicuous gap. Published with the aid of grants from the University of Cape Town and the South African Council for Scientific and Industrial Research, the *Flora* may be favorably "compared with the regional floras of many parts of the northern hemisphere."

The Introduction (10 pp.) covers such topics as area, physical features, climate, vegetation, alterations and changes, protection of the flora, the flora, arrangement, keys, descriptions, non-indigenous species, nomenclature, subdivision of species, hybrids, distribution and habitat, flowering season, popular names, contributors, and assistance rendered by specialists. According to the editors, the area involved (see maps on end papers) equals about 182 sq. miles and is roughly one-sixth larger than the Isle of Wight. After years of field work, the editors (and principal authors) and the 9 contributors admitted a total of 2,622 species in 702 genera, all substantiated by herbarium records. The style adopted throughout the *Flora* is not truly popular, yet "not overloaded with detail or with technicalities." The sequence of families is in accord with the Engler system. A notable feature is the fact that the descriptions are based on living plants, because previously published descriptions were too often drawn up from herbarium material only. The scientific names follow, as far as possible, the International Rules of Botanical Nomenclature. The origin and meaning of generic names are indicated. No attempt has been made to provide common names for all species, as only common names definitely established by usage are given. The treatment of species is not burdened with extensive accounts of infraspecific entities. Field observation has disclosed many natural hybrids, few of which have so far been studied experimentally. These hybrids are not given nomenclatural status, rather they are discussed under their parental species. Novelties discovered during field work for the *Flora* have been published in the *S. African Journal of Botany*. Expansion of real estate enterprises and industry as well as intensive grazing and the introduction and spread of plants like the Australian wattles do their share in reducing the native flora and the area now occupied by it. Fortunately, some parts of the Peninsula, like the Cape of Good Hope, have recently been set aside as Nature Reserves. A short Glossary, and Indices to the Common and Scientific Names are useful aids. A short bibliography of important works pertaining to the plant life of the area would have been helpful to those not familiar with the region.

THEODOR JUST

THE FLORA OF BIC AND THE GASPE PENINSULA, QUEBEC. *Nat. Mus. Can., Bull. 115, biol. Ser. No. 39.*

By H. J. Scoggan. National Museum of Canada, Ottawa. $1.00 (paper). 399 pp. + 1 pl. 1950.

One of the most famous peninsulas in North America in the history of plant geography is the Gaspe, first reported upon by John Macoun in 1883. The Gaspe is about 150 miles long and reaches 88 miles in width at its greatest diameter. Apropos of its botanical prestige it is not surprising to find its highest summit called Botanist's Dome (elev. 4,230 feet). The Appalachian Mountains come to their northern terminus in the Shickshock Range of the peninsula. The temperature for the peninsula as a whole ranges from a July average —the warmest month—of 63.7°F. to a January average —the coldest month—of 9.9°F. The yearly precipitation for the Gaspe is 34.7 inches, distributed pretty evenly throughout the year. It "is a region of transition between the eastern mixed forest and the northern conifer forest, between the Canadian and Hudsonian biotic provinces, and between the humid microthermal and taiga climatic provinces. In addition, elements of tundra vegetation are found on the summits of the higher mountains, and many plants of arctic-alpine character are found near sea-level in cliff and river gravel habitats."

In this thorough analysis of the Gaspe flora, Scoggan reviews the physiography, geologic history, climate, soil, and biotic factors of the peninsula. Perhaps the biotic factors are slanted toward the edaphic aspects of those factors, with little or no mention of such strictly biotic subjects as pollinating agents, seed dispersal mechanisms, obligate pathogenic fungi and predatory insect relationships, wildlife influences, and similar biotic factors that must affect the distribution of plants there. Five of Raunkiaer's life forms are represented in the flora, with an emphatic hemicryptophytic spectrum. Chamaephytes represent 9 per cent of the Gaspe flora, whereas southwestern Quebec has 4.8 per cent of chamaephytes, thus demonstrating the "rather specialized climatic features of the peninsula."

Of the total of 1216 species in the Gaspe flora, exactly 1000 are native, belonging to 418 genera. The largest family is the Compositae, with 95 species among 40 genera, as against 564 species among 82 genera in Fernald's *Manual* range. There are 77 species of grasses, the second largest family, among 39 native genera; by comparison, there are 389 species among 75 genera in the N. E. United States. The Ericaceae shows an interesting emphasis in the Gaspe, with 37 species distributed among 20 genera, the same number of genera as found in the N. E. United States, where 70 species occur. Similarly, in *Salix* and *Populus* there are 30 species in the Gaspe, 52 in Fernald's *Manual* range. Other interesting facts could be extracted from the many tables Scoggan presents. In the table of geographi-

cal affinities of the Gaspe flora it will be noticed that about 40 per cent of the native species are either arctic or boreal in their affinities, with the remainder belonging principally to temperate circumpolar, temperate American, boreal Eastern American, or temperate Eastern American categories. These subdivisions, though defined fairly closely by Scoggan, are not easy of differentiation without frequent generalized maps for the plotting of data, and the one conspicuous lack in this otherwise capital treatment is maps. Scoggan recognizes two subdivisions, the "amphi-Atlantic" (e.g., *Spartina patens, Atriplex glabriuscula,* and *Ligusticum scoticum* are representative species) and "disjunct American" (e.g., *Cheilanthes siliquosa, Galium kamtschaticum,* and *Lonicera involucrata*), the latter called "Cordilleran" by some authors, that constitute about 8 per cent of the native flora. Keys to the genera, and species, habitat, range statements in detail for local Gaspe stations and extralimital distribution, references to monographic studies, and critical synonomy make this floristic survey a valuable contribution to North American plant geography. The volume cannot avoid becoming out of print soon at the very reasonable price set by the Government of Canada.

JOSEPH EWAN

PLANT GROWTH AND DEVELOPMENT

AN INTRODUCTION TO THE EMBRYOLOGY OF ANGIOSPERMS. *McGraw-Hill Publications in the Botanical Sciences.*

By P. Maheshwari. McGraw-Hill Book Co., New York, Toronto, and London. $6.00. x + 453 pp.; ill. 1950.

Various phases of gametogenesis, fertilization, and embryogeny have been dealt with in reviews during the past decade and a half, but not since Schnarf's monographs in 1929 and 1931 has the entire gamut of events leading to seed formation in the angiosperms been handled in a comprehensive fashion. Professor Maheshwari's book, therefore, fills an obvious gap in the botanical literature at the same time that it makes available to the English reader much of the early work that has been buried in European and Asiatic journals. The book, however, is more than an embryological treatise. All events, from the early development of sporangia through gametophyte formation, fertilization, and embryogeny, are treated in a detailed and exhaustive manner. Later chapters go into apomixis, polyembryony, taxonomic relationships as reinforced or disproved by a comparison of modes of seed development, and experimental studies through embryo culture and induced parthenogenesis and parthenocarpy. The author, who has been a major contributor to the field, has done a service in thus bringing into one volume the descriptive, phylogenetic, and experimental aspects of a botanical subject which has not received the attention it deserves and which offers much excellent material for experimental studies. The book cannot be regarded as a textbook because of the narrowness of the subject matter and the detailed references which make for awkward reading, but its usefulness as a reference work is evident.

C. P. SWANSON

PLANT MORPHOLOGY

THE RHODODENDRON LEAF. *A Study of the Epidermal Appendages.*

By John Macqueen Cowan. Oliver & Boyd, Edinburgh and London. 21s. xii + 120 pp. + 16 pl. 1950.

The story goes that the late Professor Thaxter of Harvard was confronted one day by an overseer of the university's Visiting Committee who interrupted him while he was working on a species of his beloved Laboulbeinales, an obscure fungal group. When asked what possible economic value such a study could have, Thaxter hesitated not a moment. "None, thank God," he replied, somewhat to the consternation of the overseer, who was a businessman. The subject of this book falls into nearly the same category: it is a labor of love, a product of painstaking effort beautifully and carefully done, its object is taxonomic clarification, and its economic value —hmmm, well, it is an interesting subject, and Cowan wanted to do it, which reasons, to an academic person, are good and sufficient.

To be serious, the species of *Rhododendron,* which are taxonomically difficult (a difficulty which, while real, does not appear to be compounded through natural hybridization), can be identified within reason by the type of indumentum on the undersides of the foliage. At the same time, however, it is recognized that the indumentum is correlated with the habitat and the stage of development, enforcing the need for caution in relying too heavily upon such diagnostic characters. Twenty-five different types of hairs or scales are classifiable, and in considering some 370 or more species, Cowan presents a reasonable argument for the use of the indumentum as an aid in getting out of a taxonomic jungle. The author has also considered in detail the development and physiology of the indumentum, each subject in itself worthy of more study, particularly where the development and inheritance of the trichomes can be approached through the use of hybrids. The finely executed drawings, together with a very readable text, makes this an outstanding volume on a minor botanical theme.

C. P. SWANSON

RECHERCHES MORPHOLOGIQUES ET PHYSIOLOGIQUES SUR LA FLEUR DES VIOLACÉES. (*Cet ouvrage a été présenté comme thèse le 22 décembre 1945.*)

By Claude Arnal. Veuve Paul Berthier, Dijon. Paper. iv + 262 pp.; ill. 1945.

PLANT PHYSIOLOGY

AN INTRODUCTION TO THE PRINCIPLES OF PLANT PHYSIOLOGY. *Second Edition.*

By Walter Stiles. Methuen & Co., London. 60s. x + 701 pp. + 5 pl.; text ill. 1950.

In form and organization, the new edition is similar to the original. However, the 14 years which have elapsed since the last edition have witnessed a considerable amount of important work in plant physiology and biochemistry. This has necessitated a considerable reworking of many sections of the book. The major alterations have taken place in those sections dealing with photosynthesis, mineral nutrition, respiration, enzymes, plant hormones, nitrogen metabolism, and water and salt relations of plants. The biochemical aspects have been somewhat expanded and represent a desirable, modern advance.

ALVIN NASON

ECONOMIC BOTANY

SPRAYING, DUSTING AND FUMIGATING OF PLANTS. *Principles and Applications. Revised Edition.*

By Walter S. Hough and A. Freeman Mason. The Macmillan Co., New York. $12.50. x + 726 pp. + 1 table; text ill. 1951.

This is not a revision but a rewriting of Mason's 1928 book of the same title. Though the principal purpose is to produce a readily consultable guide to the principle and practice of the use of chemicals for the protection of plants against insects and fungi, the scope of the book is not limited to this subject. There is included a chapter on the use of growth regulators to promote root growth in cuttings, to improve yield, and to delay fruit drop; and another on the use of herbicides.

The production in the last decade of a multitude of chemicals more or less well adapted to pest control, and the extensive testing of these for their effectiveness, has created an enormous literature. In bringing together in one volume the chief usable results of this mass of work the authors have filled an urgent need. Despite the presence of much interesting historical information on fungicidal and insecticidal chemicals and considerable emphasis on the principles of their use, the book is written primarily for the man who needs to control plant pests. Every possible guidance is given in the form of tables of specific recommendations against particular pests. Though the price is high, purchase of the book will be a sound investment for large-scale growers of fruit, vegetable, and other crops that can be protected against insects and fungi by the application of chemicals.

T. JOHNSON

PRINCIPLES OF WEED CONTROL.

By Gilbert H. Ahlgren; Glenn C. Klingman and Dale E. Wolf. Wiley & Sons, New York; Chapman and Hall, London. $5.50. viii + 358 pp.; ill. 1951.

This book was written primarily for use in an undergraduate course on weed control, and the early chapters stay on this level. However, the later chapters cover many of the more important weed-control problems and are written for the more informed person.

Included are discussions of mechanical and cultural weed control practices as well as the chemical techniques. The book is copiously illustrated and has over 400 references. Although the book was issued in the fall of 1951, relatively few 1950 references are cited. Some of the more recent review articles on herbicides could have been included to good advantage. Illustrations include weed-control equipment, views of treated and untreated areas, and the common weeds. The usual lists of weeds and their susceptibility to 2,4-D are also given.

On the whole, the book is well written and is the only up-to-date volume on this subject. It should be useful as a textbook and also as a ready reference for all who are interested in practical weed control.

C. E. MINARIK

DISEASES AND PESTS OF ORNAMENTAL PLANTS. *Revised Edition.*

By Bernard O. Dodge and Harold W. Rickett. Ronald Press, New York. $6.00. xiv + 638 pp.; ill. 1948.

The reception given to the earlier (1943) edition of this book (*Q.R.B.*, 19: 157, 1944) has prompted a revision. Presupposing the minimum of technical knowledge of botany and entomology, this book presents under one cover the information necessary for the diagnosis of the diseases and pests affecting all of the important ornamental plants of the United States, and recommends practical control measures.

Part One, introductory in nature, deals with the classification of plant diseases, and the general nature of bacteria, fungi, insects, and other animal pests. The chapter on control is particularly well done, emphasis being placed on the rationale of various control measures rather than on blind reliance upon cookbook recipes.

In Part Two, comprising almost 500 pages, the diseases and pests of particular hosts are considered. From *Abelia* to *Zinnia*, some 600 host species are included. Although the hosts are arranged according to their scientific names, an index of the common names is included for ready reference.

The book is amply illustrated with some 200 photographs, almost all of which are original. The many years of experience in plant pathology of the senior author have been combined with the organizational talents of the junior to yield a product which is up to

date yet conservative in approach, and authoritative without sacrifice of practicality.

F. T. WOLF

A FUCHSIA SURVEY.

By W. P. Wood. Williams & Norgate, London. 9s. 6d. xii + 170 pp. + 9 pl.; text ill. 1950.

Although of western hemisphere origin, the fuchsia, as a garden plant, is typically British. Only the rose compares favorably with it in general popularity. In this country, it can be grown to its full luxuriance only in the frost-free states; elsewhere the fuchsia is a house or bedding plant, and it has never achieved the favor or prominence with which the British regard it. The present book, covering the historical and cultural phases of fuchsia growing, should serve to stimulate the interest of gardeners in this unusual plant.

C. P. SWANSON

ALL ABOUT AFRICAN VIOLETS. *The Complete Guide to Success with Saintpaulias.*

By Montague Free. The American Garden Guild and Doubleday & Company, New York. 304 pp.; ill. 1951.

This book is the answer to the many problems of all those who have tried growing the capricious *Saintpaulia* and had indifferent or poor results. The author has written from his own experiences and has used illustrations of plants grown in different home locations. The information is presented in a logical and non-technical style. Light, moisture, and soil conditions are given for the care of plants from early propagation to the mature plant. The chapter, Trouble: Avoidance and Cure, presents many of the difficulties encountered under home conditions and their correction. Numerous photographic and beautiful water-color illustrations are used throughout the book. A glossary of terms, a list of the species and varieties with brief distinguishing characteristics of each, and a complete index add much to its value.

HELEN S. WILLIER

GENERAL AND SYSTEMATIC ZOOLOGY

ANIMAL BIOLOGY. *3rd Edition.*

By A. J. Grove and G. E. Newell; foreword by C. F. A. Pantin. University Tutorial Press, London. 21s. xii + 760 pp. + 9 pl.; text ill. 1950.

American teachers of biology, unfamiliar with their English counterparts, will find much of interest in this English textbook designed for students of biology and medicine. The lucid and straightforward text is well integrated with the numerous illustrations, and the treatment, although phylogenetic in essence, lays its greatest emphasis on the relationship of structure to function. In addition, the chapters on behavior, cytology, genetics, embryology, and evolution are generally superior in content and presentation to those more commonly found in our own textbooks, although one must note a rather marked deficiency in physiological and biochemical subjects. Clearly the English expect more of their elementary students than we do, with the consequence that their writers of beginning textbooks have no hesitation in introducing theory as well as facts for memorization.

C. P. SWANSON

GUIDE TO SOME COMMON COLORADO SPIDERS. *Leaflet No. 8, Univ. of Colorado Museum.*

By Walker Van Riper. University of Colorado Museum, Boulder. 25 cents (paper). 24 pp. + 12 pl. 1950.

"Unlike most other leaflets in this series, this is not a guide for the identification of all the spider species found in Colorado. Our species are too numerous and have been too little studied to make a complete guide possible. The aim of this leaflet, instead, is to provide a sort of introduction to spiders by describing and picturing a few of the most conspicuous and familiar species." With these words the author prefaces his remarks about the 30 species he has selected. These belong to 8 different families. Descriptions are kept to a minimum, but there are many details concerning the life and habits of his interesting subjects. To a great extent these details are based upon the author's own personal observations, and there are thus added valuable bits to our knowledge of spiders. The pamphlet includes 24 exceptionally fine photographs taken by the author himself by means of the high speed flashlight method for which he is justly famous. Unfortunately the reproduction of these (by the offset method) is such as to detract somewhat from their clarity. The pamphlet should serve its purpose well, and will no doubt spur many of its lay readers to seek further information in this area of natural history.

B. J. KASTON

THE ARACHNID ORDER SOLPUGIDA IN THE UNITED STATES. *Bull. Amer. Mus. nat. Hist., Vol. 97, art. 2.*

By Martin H. Muma. American Museum of Natural History, New York. $1.25 (paper). Pp. 31–142; ill. 1951.

Except for a 1-page introduction, this monograph is purely taxonomic. There are well constructed keys to all systematic categories, and descriptions that are quite

detailed for the most part. Over 300 fine drawings illustrate the more important characters. In addition to the description, the author has supplied distribution notes for each of the 82 species and some comments concerning relationships with related species.

In a spirit of helpfulness the author supplies for every specific name a footnote giving its derivation. This is useful and interesting, but would appear to be superfluous in the case of patronymics (viz., "*gertschi*," "*mulaiki*," "*morrisi*," etc.) and place names (e.g., "*texana*," and "*californicus*," etc.), especially as the derivation would be quite obvious from the locality and collecting records supplied.

Since there has been no detailed faunistic publication on solpugids for any region of North America since 1883, Muma's thorough work is a welcome addition to the literature and will long stand as a useful reference to make the task of later workers much easier.

B. J. KASTON

A MONOGRAPH OF THE COLLEMBOLA *or Springtail Insects of New York State.*

By Elliott A. Maynard. Comstock Publishing Co., Ithaca, N. Y. $7.50. xxiv + 339 pp. + 5 pl.; text ill. 1951.

The title indicates the scope of the monograph. The author describes 200 species, of which 57 are new species and new forms. Four new genera are added.

Part I consists of 19 pages devoted to a description of the faunal districts (as established by Forbes, 1926), a phylogenetic tree, the external and internal structure of the Collembola, their life cycles, habitats, food, and geographical distribution. There is a brief note on nomenclature and synonymy in which Maynard states that he follows the classification and nomenclature of Gisin (1944). Unfortunately the author lists 4 titles for Gisin in the year 1944, at least two of which appear important.

Part II, the remainder of the book, is devoted to a systematic account of all the species recorded from New York State. Unfortunately the writer does not follow good taxonomic procedure. The various species are correctly indicated by their authorities. However, the references to these authorities are merely listed by name and year. For example, *Podura aquatica* Linnaeus has Linnaeus 1756 as the original describer, but the literature has no reference for Linnaeus of that date. Again *Hypogastura harveyi* Folsom has Folsom, 1902, as the describer. Unfortunately, Folsom has three papers assigned for that year. When some of the authors have as many as 5 papers for the year assigned, it is most unfortunate that the writer does not give the exact reference with the name of the describer, the publication, date, and page for each species. The numerous other references might stand as indicated by year, even though this is not accepted taxonomic procedure. It follows that any student desirous of determining the taxonomic history of any species must search through all the papers of, in most cases, many authorities, and this is dreary work. However, the descriptions and figures of so many species will be of great aid to the beginner as well as the seasoned worker. The colored plates are beautifully done and well reproduced; the line drawings are excellent.

The book concludes with a long bibliography of the Collembola of the world and an index. The make-up and book work is of high grade, and the printing all that could be desired.

ROBERT MATHESON

ATLAS DES DIPTÈRES DE FRANCE (BELGIQUE-SUISSE). *Two Volumes. I: Introduction et Caractères Généraux, Nématocères-Brachycères I. II: Développement et Biologie. Brachycères II-Siphonaptères.*

By E. Séguy. N. Boubée & Cie., Paris. 750 fr. per volume (paper). I: 175 pp. + 12 pl.; text ill. II: 185 pp. + 12 pl.; text ill. 1951.

The two fascicles comprising this atlas of flies will be welcome and useful additions to the libraries of general biologists and entomologists whose research or interests lead them to occasional work with scattered members of the order Diptera. The 450 and more different illustrations, fully 60 per cent of which are colored, portray the habitus and distinguishing morphological characters of imagines (and occasionally also of larvae) from all of the more typical families of flies of the north temperate world. The pen figures are well delineated, and nearly all of the colored illustrations are very good. Collectively they should prove of very considerable service to novices attempting to place specimens of flies with Brues' and Melander's *Classification of Insects*, or similar manuals. In this respect these fascicles will prove much superior to Pearce's three series of *Typical Flies*.

The texts include general remarks about flies and their collection, brief accounts of the internal and external anatomy of adult and larva, a very brief introduction to the biology of flies, short characterizations of the suborders, superfamilies, and families, and thumbnail sketches of each fly illustrated, and generally including hints as to when and where they may be collected. Concluding the second fascicle is a similar treatment, 20 pages in length, of the fleas of France.

KENNETH W. COOPER

THE GENUS CHEILOSIA MEIGEN (DIPTERA, SYRPHIDAE). *The Subgenera Cheilosia and Hiatomyia. Bull. amer. Mus. nat. Hist., Vol. 94: Art. 6.*

By Frank Montgomery Hull and Charles Lewis Fluke,

Jr. American Museum of Natural History, New York. $1.25. Pp. 299–402; ill. 1950.

Two of the leading North American authorities on the syrphids have collaborated to present in this paper a comprehensive study of the world fauna of one of the more difficult groups within this family. Generic and subgeneric limits are defined in detail and valuable notes on synonymy are included, along with concise redescriptions or descriptions of more than 90 previously known or new species. Extensive keys to *Cheilosia* (s. s.) and *Hiatomyia*, together with accurate drawings, will enable students to readily identify species in these groups.

NEW AND LITTLE KNOWN AMERICAN TACHINIDAE. *Bull. amer. Mus. nat. Hist., Vol. 89: Art. 2.*

By C. H. Curran. American Museum of Natural History, New York. 95 cents (paper). Pp. 41–122; ill. 1947.

Twelve old genera and 1 new genus of this notoriously difficult family are treated by Curran in this paper. Seventy-one species are described as new and notes or remarks on 43 previously known species are given. Specific keys are also included for 8 of these genera. This paper will probably prove very useful to students of this family, particularly those who are interested in the classification of the Neotropical Dejeaiini.

SPECIES OF EUXOA OF EASTERN NORTH AMERICA, WITH PARTICULAR REFERENCE TO GENITALIC CHARACTERS (LEPIDOPTERA, PHALAENIDAE). *Bull. amer. Mus. nat. Hist., Vol. 95, art. 6.*

By James H. McDunnough. American Museum of Natural History, New York. 75 cents (paper). Pp. 355–408; ill. 1950.

THE SHELL COLLECTOR'S HANDBOOK.

By A. Hyatt Verrill. G. P. Putnam's Sons, New York. $4.00. xv + 228 pp.; ill. 1950.

The need for a good handbook for the collectors of shells has long been felt, but the present work leaves the need unfilled. The author is a commercial dealer, and his mind was on his clients as he wrote. The book is quite obviously addressed to those who are likely to develop into Boltens, Tankervilles, or Yoldis, rather than to the incipient Rodings, Sowerbys, and Mörchs.

There are not many dealers in shells in this country today, but there are more than a generation ago, which would seem to indicate that the modern collector is coming to rely more on his pocketbook than on his eyes and hands in building a collection. Not that the author does not appreciate the importance of field work. He has done a great deal of field work himself—he had to in order to build up his business. But it is inevitable that a book which is the product of commerical activity will tend to place the emphasis upon the acquisition of material rather than upon the acquisition of knowledge.

There is no original contribution to the science of malacology in this work. Indeed, the author had no such intention. In fact, the only strictly original feature is the somewhat bizarre orthography of scientific terms to which the author is addicted. Some printers are notoriously careless, and in books which are the victims of incompetent proof-reading some typographical errors creep through; but in this work the errors are so carefully standardized that they cannot be attributed to the typesetter. For this reason, this book cannot be recommended to the beginner, for it contains so much that the beginner would have to unlearn later. This is doubly unfortunate, as it was apparently for the edification of the beginner that the book was written. It is essentially an attempt to popularize science by rewriting in simplified language such passages, assembled from other works, as seemed to the author worthy of this honor, but using words of one syllable where the original author had found words of two syllables to be necessary. [The reviewer knows, for he chanced to find among these selected passages half a chapter from an earlier work of his own, published a decade or so ago, and rewritten in this way.]

Perhaps it was such a popularization of science that impelled Kipling to write:

> "When 'Omer smote 'is bloomin' lyre
> 'E'd 'eard men sing o'er land and sea,
> And what 'e thought 'e might desire
> 'E went and took, the same as me."

JOSHUA L. BAILY, JR.

STUDIES OF CYPRINODONT FISHES. *XX. A New Subfamily from Guatemala, with Ctenoid Scales and a Unilateral Pectoral Clasper. Misc. Pub. Mus. Zool., Univ. Mich., No. 78.*

By Carl L. Hubbs. University of Michigan Press, Ann Arbor. $1.25 (paper). 28 pp. + 4 pl.; text ill. 1950.

The new species, *Xenodexia ctenolepis*, from the Rio Usumacinta, is so highly specialized as to require the erection of a new genus and distinct subfamily, the Xenodexiinae. It has "an elaborate unilateral pectoral-fin clasper of the adult male—a structure unique among fishes; ctenoid scales, hitherto unknown and unpredicted in the order Cyprinodontes; and an extreme extension of the body cavity into the urosome." It is carefully described and beautifully illustrated.

VARIATION OF PEROMYSCUS MANICULATUS IN PARTS OF WESTERN WASHINGTON AND ADJACENT OREGON. *Contrib., Lab. Vert. Biol., Univ. Mich., No. 44.*

By Lee R. Dice. University of Michigan Press, Ann Arbor. 35 cents (paper). 34 pp. 1949.

RESULTS OF THE ARCHBOLD EXPEDITIONS. NO. 59. STUDIES ON THE ANATOMY AND PHYLOGENY OF THE MACROPODIDAE (MARSUPIALIA). *Bull. amer. Mus. nat. Hist., Vol. 91: Art. 2.*

By G. H. H. Tate. American Museum of Natural History, New York. $1.50 (paper). iv + pp. 237–351; ill. 1948.

Although mainly anatomical, this study includes valuable notes on the geographic distribution and phylogeny of the 42 good species of kangaroos and wallabies and the numerous races and subspecies which have been described. The maximum number of species in any one region is 16 in E. New South Wales, E. Queensland has nearly as many, Victoria and part of W. Australia have 11 each, but elsewhere the number is much reduced. New Guinea has only 7 species in all, and only 4 in any one region. Members of the family are primarily lowland animals, although in New Guinea wallabies may go up to 11,000 feet. On the other hand, kangaroos and wallabies are not well adapted to arid regions, and the vast central region of Australia has very few representatives of her most characteristic animals. It was probably different in the rainier portions of the Pleistocene.

ECONOMIC ZOOLOGY

GALL MIDGES OF ECONOMIC IMPORTANCE. *Vol. V.—Trees.*

By H. F. Barnes; foreword by R. Neil Chrystal. Crosby Lockwood & Son, London. 15s. 270 pp. + 8 pl. 1951.

The earlier volumes of this work have already been reviewed in this journal (*Q.R.B.*, 23: 63; 24: 61; 25: 89). They dealt with the Gall Midges of Root and Vegetable Crops (Vol. I), Fodder Crops (Vol. II), Fruit (Vol. III), and Ornamental Plants and Shrubs (Vol. IV). Future volumes will deal with species attacking Miscellaneous Crops (Vol. VI), Cereals (Vol. VII), and with predaceous and fungivorous species and those which attack weeds (Vol. VIII). The complete opus, which will presumably include over 1500 pages, will constitute a comprehensive review of the biology of an extremely interesting but much neglected family of the Diptera. H. F. Barnes, who is Entomologist at the Rothampsted Experimental Station in England, has been for many years the chief world authority on this group of insects.

The plan of the work is to take the economically important plants, in alphabetical order in each volume, and to discuss each of the species of cecidomyids known to form galls on it. Thus the book has some of the characteristics of a dictionary, with both the advantages and disadvantages inherent in such a plan. It is not a taxonomic work, although full references to the original descriptions of the species are given. Emphasis is on the biology of the insects; full details of the life history, distribution, natural enemies, and range of host plants (where these are known) are given for each form. Short notes on control measures which have been employed in the past are also included in many cases.

On account of their minute size the gall midges (Cecidomyidae) are one of the least known families of insects, the number of undescribed species, even in western Europe and the northeastern states of the U. S. A., being very great. Knowledge of the biology of many of the commonest species is extremely fragmentary. In a great many cases a species is known only by its larva and the gall which it produces, the adult insect being still unknown. Presumably the trees of tropical forests suffer as much from cecidomyids as those of Europe and North America, but with very few exceptions, hardly any tropical trees are mentioned in the present volume.

Restriction of the book to forms of economic importance has led to some rather curious omissions. Thus to qualify for detailed treatment in this work a cecidomyid has to (1) attack a tree of economic importance and (2) be at any rate potentially capable of causing financial losses to the owner of the tree. Thus the hackberry midges are not dealt with, presumably because the author does not consider *Celtis* spp. sufficiently valuable trees, and the very interesting species of hickory midges (genus *Caryomyia*) are barely mentioned because they are not regarded as likely to cause significant damage to the admittedly valuable trees which they attack.

From the standpoint of the general biologist the most interesting sections of the present volume are those dealing, first, with the pitch midges, whose larvae live in masses of resinous material exuding from the twigs of various species of pines, with only the extremity of the abdomen, bearing the posterior spiracles, protruding; and, second, with the midges which attack willows. The latter, which mostly belong to the genus *Rhabdophaga*, have been studied in detail by Barnes, who has shown that at least some of the species produce unisexual broods. Unfortunately this section exemplifies one of the most annoying features of the whole work, since it includes only those species of willows used for making cricket bats, the cecidomyids which attack basket willows being apparently dealt with in Vol. VI. Extensive cross-indexing does not compensate for this excessive fragmentation of the biological material according to the particular economic uses of the plants which are attacked.

The 8 plates in the present volume portray several of the species attacking pines and willows, as well as showing characteristic types of damage. There are very few illustrations of galls. For American readers this does not seriously detract from the value of the book, since we have E. P. Felt's copiously illustrated *Plant Galls and Gall Makers* to consult.

Certain general problems of considerable importance are hardly touched upon by Barnes. One is the vexed question of natural immunity to cecidomyid attack. The reviewer vividly remembers two yew trees near London which grew so close together that their branches interlaced. Year after year one was covered with tens of thousands of galls of *Taxomyia taxi*, while the other carried hardly any. Similar observations have been made on hackberry galls (due to *Phytophaga celtiphyllia*) on the campus of the University of Texas. Presumably, in such cases we are dealing with genetically determined differences in resistance to cecidomyid attack, but the actual mechanisms involved are not understood. Ordinarily, very few of the cecidomyids which make galls on trees constitute serious pests, but occasionally species usually regarded as innocuous may cause quite heavy damage (one or more species of *Retinodiplosis* have caused stunting or death of many Ponderosa pines in California, Oregon, and Idaho).

The bibliographies of Barnes' volumes are a very special feature of the work and testify to the erudition of the author in his particular field. The literature list of the present volume includes 445 titles in at least a dozen languages (Barnes seems just as familiar with the literature published in Finnish, Danish, or Portuguese as he is with papers in English). Minor typographical errors do exist, however, in references nos. 300, 338–340, 382, and 410.

M. J. D. WHITE

POULTRY HUSBANDRY. *New Third Edition. McGraw-Hill Publications in the Agricultural Sciences.*

By Morley A. Jull. McGraw-Hill Book Co., New York, Toronto, and London. $6.00. viii + 526 pp. + 1 folding chart; text ill. 1951.

This third edition of a well-known textbook fulfils in a very satisfactory manner its purpose, which is "to convey to its readers some conception of the relative importance of the poultry industry as a branch of agriculture, the fundamental principles involved in various poultry practices, and up-to-date information concerning methods of poultry production."

The book is some 20 pages shorter than the 2nd edition, but the condensation, which has permitted the inclusion of a new chapter on the poultry industry, is an improvement. Of the 253 illustrations, many are new, and the old ones show up much better than in earlier editions because of the fine white paper used this time. The 14 chapters deal with breeds, biology of the chicken, principles and practices of breeding and of feeding, incubation, rearing chickens, housing, diseases, the marketing of eggs and poultry, and the economics of different branches of the poultry business. In all of these fields, the author endeavours to summarize the present state of knowledge and to show the relation of principles to practice. After contemplation of the series of ghastly pictures showing what happens to poor little chicks that do not get enough arginine, glycine, vitamin A, thiamine, choline, folic acid, or other essential constitutents of the diet, one wishes that Jull had reassured his readers by explaining that the horrible plight of the chicks shown was induced by deficient diets specially devised for laboratory experiments, and that ordinary chickens raised on table scraps and lawn clippings in a back yard, or provided with a modern ready-mixed mash, are not likely to fall into such harrowing states of disrepair. In general, the book should serve as a satisfactory textbook for an introductory college course in poultry husbandry. There are references to pertinent literature after each chapter, and an index.

F. B. HUTT

BREEDING AND IMPROVEMENT OF FARM ANIMALS. *Fourth Edition.*

By Victor Arthur Rice and Frederick Newcomb Andrews; with Chapter on Selection in Meat Animals by Everett James Warwick. McGraw-Hill Book Co., New York, Toronto, and London. $7.00. xiv + 787 pp. + 2 folders; text ill. 1951.

V. A. Rice and his associates have organized a rather large vertical slice of biology, especially of the domestic animals. One must assume that students who will use this book begin in ignorance and complete the textbook in full possession of all salient facts and concepts in the field. The book is indeed excellent. The principles and mechanisms of reproduction and heredity are presented adequately. The authors achieve a nice balance between physiology and genetics on the one hand, and management and subjective selective breeding on the other. The book is highly recommended for student, teacher, breeder, and casual reader, and for use as a reference.

T. C. BYERLY

FARMING FOR BEEF.

By Allan Fraser. Crosby Lockwood & Son, London. 10s. 6d. 144 pp. + 12 pl. 1950.

The political and technical aspects of beef production are discussed. The author develops the thesis that governmental policies in Great Britain during the past decade have been directed almost exclusively toward milk production. He writes: "We have, to a certain extent, discarded the substance of beef for the shadow

of milk. He traces the development of beef-type cattle, the origin of the British beef breeds, their characteristics and native habitat. Fraser does not suggest the elimination of dairy cattle, but he points out the value of dual-purpose cattle in meeting both fluid milk and meat needs. He stresses the value of beef cattle in the utilization of marginal land and discusses the production of beef with minimal amounts of feed grains. The book is written primarily for the home food producer, but the historical and political aspects will interest professional cattle producers.

FREDERICK N. ANDREWS

DEER AND THEIR MANAGEMENT IN THE DEER PARKS OF GREAT BRITAIN AND IRELAND.

By G. Kenneth Whitehead. Country Life, London. £4 4s 0d. 370 pp.; ill. 1950.

In the modern American literature on wildlife management, the word "management" usually connotes working for a desirable balance of wild species under conditions as nearly natural as possible. In Whitehead's very British book, the management of deer is more like what Americans would call animal husbandry, with building up and maintenance of herds of native and exotic cervids in parks, transfers of stock from one place to another, artificial feeding, culling, and harvesting meat and trophies.

The histories of deer parks reflect a phase of European culture that is now adhered to with much difficulty. The chapters on species have considerable zoological interest—though it does seem strange to read about the red deer without seeing any reference to F. Fraser Darling's classic behavioristic study of that species. Possibly the outstanding chapter is the one on the diseases and afflictions of deer, and, in this, the many pages devoted to rabies contain especially interesting information, a great deal of which is unlikely to be available elsewhere.

PAUL L. ERRINGTON

ANIMAL GROWTH AND DEVELOPMENT

THE NEURAL CREST. *Its Properties and Derivatives in the Light of Experimental Research. Based on a Series of Special University Lectures Delivered Before the University of London.*

By Sven Hörstadius. Oxford University Press, New York. $3.00. x + 111 pp.; ill. 1950.

This most interesting small volume of 5 chapters devoted to the properties and derivatives of the neural crest is based upon a series of special lectures on the subject delivered by Professor Hörstadius, of the University of Uppsala, at the University of London in 1947. Actually it is an excellent comprehensive review of the experimental work which has been done on the neural crest of the various classes of vertebrates within the last three or four decades. Material from approximately 250 original papers, which has not heretofore been brought together, is presented with clarity and directness. Since the main purpose of the author (himself an outstanding experimental embryologist) is to show students of biology that knowledge in this particular field has been obtained almost entirely by the use of experimental methods, he presents the lines of thought and the methods that experimental embryologists have used to establish the origin and properties of the many and varied neural crest derivatives.

In view of the fact that authors of modern textbooks of embryology and anatomy make only passing mention of this extremely interesting and complicated transitory embryonic structure, this book is, indeed, most welcome. It is very gratifying that such important information has been gathered together by so competent a person and made so easily available to students and teachers of biology.

MARY E. RAWLES

ANIMAL MORPHOLOGY

THE INTEGUMENT OF ARTHROPODS. *The Chemical Components and their Properties, the Anatomy and Development, and the Permeability.*

By A. Glenn Richards. University of Minnesota Press, Minneapolis; Geoffrey Cumberlege, Oxford University Press, London. $6.00. xvi + 411 pp.; ill. 1951.

The appearance of this excellent book is another indication of a long-continued trend away from the view that insects constitute a special group with little relation to the rest of the animal kingdom and that entomology is something of interest chiefly to collectors. The author is an entomologist and one of that small heterogeneous group, known as insect physiologists, which in the United States meets annually under the encouraging auspices of The Entomological Society of America.

This is no mere compilation of available literature. A carefully thought out and critically evaluated picture of the present status of knowledge of the arthropod, particularly the insect integument, is presented. The integument is viewed as a physicochemical, an anatomical, and a physiological structure or organ. Section I (109 pp.) deals with the chemical components, their combinations, and properties, with greatest emphasis on chitin and related compounds, proteins, lipids, pigments, and certain inorganic constituents and enzymes. Both chemical and physical properties are discussed. Section II (122 pp.), on microanatomy and development, describes and discusses the several subdivisions of the integument and includes such related topics as physical colors, oenocytes, and tracheae. Section III

(23 pp.) is on the permeability of the cuticle, particularly the penetration of water, gases, electrolytes, nonelectrolytes, and insecticides. Evidently this section is shorter because the author found "valid permeability data" none too plentiful.

This book is well written, informative, and interesting. It is recommended highly to anyone active in this or related scientific areas and to all who are interested in gaining in the shortest time the fullest conception up to 1950, of the arthropod integument. Seventy-six pages of references will direct the researcher to the original literature.

J. FRANKLIN YEAGER

FUNCTIONAL ANATOMY OF THE VERTEBRATES. *McGraw-Hill Publications in the Zoological Sciences.* *By Daniel P. Quiring. McGraw-Hill Book Co., New York, Toronto, and London.* $5.50. xii + 624 pp. + 1 pl.; text ill. 1950.

This book represents an attempt to present comparative vertebrate anatomy from what the author chooses to call a functional point of view. What this appears to amount to is that some aspects of function and behavior of vertebrates are correlated with certain morphological data, which consist largely of the relation of the weight of particular organs to total body weight in various species. These data are presented in the text in the form of tables and graphs and summarized in an extensive appendix. One example of the way in which this kind of information is employed is afforded by its utilization to illustrate the author's concept of "levels of being." Thus, if a log plot is made of brain weight vs. body weight, it is found that the resulting straight lines, each representing a different group of vertebrates, occupy different levels on the graph. Similar graphs being constructed for other systems, it may be shown, according to the author, that animals experiencing the greatest development of organs controlling energy release occupy the highest positions on such a life scale. Key organs in this respect are considered to be the brain, adrenals, thyroid, heart, liver, and lungs.

Further exposition of what is meant by "levels of being" leaves the impression that the individual animal's level of being is in reality a definition of its species. Applied to groups of animals it would serve to define genera, families, and other subdivisions of the animal kingdom. Thus, it is stated that animals live on specific plateaus determined by their degree of development. The attributes which determine an animal's position with reference to its environment are fixed by its genetic constitution. It is pointed out that possibility for change is inherent in the genetic mechanism through the capability to undergo mutation. The presentation is formally correct, but it seems that there are mystical overtones in the author's presentation of his ideas, not only in his discussion of levels of being, but in other connections as well. This impression of mysticism is sometimes conveyed by the frequently elaborate phraseology which characterizes certain portions of this book. Sometimes, however, they are implicitly expressed. Thus, in concluding a brief discussion of neural activity, the remark is made that "it is difficult to reconcile oneself to the idea that mental activities, including rational thought, are nothing more than chemicophysical disturbances." The implication is quite obviously that the author has not reconcilled himself to this idea. There is a certain redundancy in those sections that elaborate what Quiring considers to be the fundamental ideas and concepts of biology, including the "levels of being."

Another criticism of this book is the existence of some rather serious errors and deficiencies attributable either to ignorance or carelessness on the part of the author or to inadequate editing. We read, for example, that "the ultimate stage of nirvana is reached . . . where heat no longer flows and complete entropy is reached (p. 17). This passage refers to the disintegration of Man, a process in which "the particles, originally filled with fire, with ruthless ambition or with high thought and striving, sink to a state of heat death, the most probable state . . .", i.e., to a state of "complete entropy."

Farther on, the author endeavors to enlighten his readers as to the nature of muscle contraction (p. 167). Here it is unfortunate that the discussion indicates only the nature of certain ideas concerning this phenomenon current in 1930. Exposition of present-day ideas would, of course, be infinitely more difficult to condense on a single page in language understandable to a student of comparative anatomy. On the other hand, there is no justification for presenting, in the guise of authoritative pronouncements, what amounts to gross misinformation.

Happily, the greater part of this book is devoted to a reasonably straightforward account of comparative anatomy. In this it leans heavily, like all present-day textbooks, upon contributions of the past. The arrangement is by systems. A separate chapter is given over to chordate classification. Several conclude with brief "sidelights." These are entertaining accounts of the author's personal experiences in the field and are intended to amplify various portions of the text with examples from nature. Some of the illustrations fall more or less into this same category. There is a photograph of the large intestine of an elephant and one of the intestines of a finback whale. The illustrations in general are adequate.

There is a short bibliography following each chapter, although not all text citations can be found in the appropriate list of references. The extensive appendix gives the body and organ weights of 3,581 vertebrates. Presumably this is not intended as required reading. There is a glossary and an index. The format is typical of the McGraw-Hill series.

MAX KRAUSS

AN ATLAS OF ANIMAL ANATOMY FOR ARTISTS.

By W. Ellenberger, H. Baum and H. Dittrich; translated from the German by Helen Weinbaum. Dover Publications, New York. $6.00. ii + 122 pp.; ill. 1949.

If one wishes to avail himself of a wealth of information concerning the comparative anatomy of the horse, dog, lion, cow, bull, stag, roe, and goat—this book is the means. Being an atlas, the full-page half-tone illustrations show the animals in normal standing positions and include their topography, superficial muscles, and skeleton plus additional drawings of head and limb details. The general plan is as follows:

The horse (lateral view) with hide, with muscles, with skeleton
Same (anterior view)
Same (posterior view)
Same (superior view)

The dog, lion, bull, and cow are treated similarly, but stag, roe, and goat receive somewhat less attention. Each plate is complete with legends.

The original works were complete anatomy texts on the various animals, including in great detail nervous and vascular systems, viscera, etc. This book emphasizes in a much better organized form only those anatomical details which would be of interest to the artist, sculptor, comparative anatomist, or preparator of mounted osteological speciments. The page size is ample, but not oversize, and the plates lie flat and in full view. Here is exterior comparative anatomy presented accurately and beautifully, so much so that one's instantaneous reaction is: "I wish there were many, many more animals." The preparing and publishing of this book was like removing the tarnish from old silver.

WM. E. LOECHEL

KINESIOLOGY. *The Mechanical and Anatomic Fundamentals of Human Motion.*

By Katharine F. Wells. W. B. Saunders Co., Philadelphia and London. $4.75. x + 478 pp.; ill. 1950.

Kinesiology is a textbook intended primarily for teachers and students of physical education, physical therapy, and occupational therapy. The reader can best decide for himself to what extent the material included serves his purpose by noting the amount of space given to each section, and the areas of chief emphasis.

Part One is devoted to a fairly brief analysis of mechanics in motion. This section discusses in a simplified way the laws of physics which govern stability and motion, and some of the mathematics relating to muscle action. The second part makes up over half the book. It deals with the anatomy and function of joints and muscles. The author intends in this and the preceding section to provide all the information regarding anatomy and mechanics that the student of kinesiology will need, and thus to avoid the necessity for using supplementary textbooks. The material in the second section is very well organized, clearly presented, and generously illustrated with excellent line drawings and photographs. However, there are a few statements of questionable accuracy. A definite error occurs in the analysis of the "gluteus medius gait. (This same error is repeated in Part Three.) The next part relates to motor skills and their basic principles. This section is devoted chiefly to detailed analyses of the mechanical and anatomical principles involved in specific sports and postural skills and in locomotor activities. There is also, a short chapter on prevention of injuries. The last part, which deals with the application of kinesiology, comprises less than a fifth of the book. In this section the author does not try to anticipate each problem the student might meet and solve it for him in advance. Instead, she provides a plan of procedure for evaluating and selecting activities, and illustrates the process with a few examples. In addition to the material already mentioned, most sections of the book include reviews of pertinent research and have a suggested list of demonstrations and laboratory exercises. Each section has a bibliography, and several have additional material listed for recommended reading.

The parts of the book dealing with mechanics and anatomy are an instructive and stimulating source of information. The suggestions for applying mechanical principles to practical situations are very interesting and worth-while. However, the part of the book which deals with the application of muscle function to sports skills and to posture is much less adequate. It is hard to understand the emphasis which is placed upon exhaustive analyses of muscle action in sport skills. It is a foregone conclusion that almost every muscle in the body acts to some extent in any active sports movement. Therefore, the point is not so much exactly what muscle acts at any given phase of the movement, but whether each muscle is capable of doing its share when its turn comes. This information can be had only by examining the performer, not the motion.

If the kinesiologist is interested in the improvement of the individual, as the author says he should be, more of his time and effort should be spent in learning to do a muscle test of the individual rather than a muscle analysis of the sport. If he knows enough anatomy to do the former, he will have little trouble in doing the latter when it is indicated.

In the discussion of therapeutic exercise, 5 exercises are analyzed and evaluated and one postural fault is analyzed. The intent is obviously to suit corrective measures to the specific needs of the individual. Unfortunately, this objective is not met fully because both the analyses of the exercises and the analysis of the postural fault are inaccurate in some important details.

In the Introduction, the author states that the student of kinesiology "must remember that the skill itself is of less importance than the one who practices

it. Yet the text emphasizes the activity almost exclusively and gives scant attention to the performer as an individual. This fact is basic to most of the errors and inconsistencies that can be found in the book. Unfortunately, it is a mistake in emphasis which accords with the tradition of kinesiology. Had Miss Wells broken with tradition in this respect, her book would have been of outstanding value.

HENRY O. KENDALL

AIDS TO HISTOLOGY. *Fifth Edition.*

By Geoffrey H. Bourne. Bailliere, Tindall & Cox, London. $1.75. viii + 158 pp.; ill. 1950.

This volume presents something of a study in contrasts. Small, thin, unattractively bound, printed on cheap paper and illustrated with line drawings which are downright crude, one is led to expect little of the text. However, a perusal of its short sections reveals a remarkably skillful and judicious concentrations of histological fact. Both histology and organology are covered. The style is simple and clear. It is entirely too short to serve as a medical school textbook but should be very valuable to those preparing for an examination after having studied the subject. Workers in cognate fields requiring only occasional reference to histology will find it an excellent refresher.

F. N. LOW

A TEXTBOOK OF HISTOLOGY. *Functional Significance of Cells and Intercellular Substances. Fourth Edition, thoroughly revised.*

By E. V. Cowdry. Lea & Febiger, Philadelphia. $8.00. 640 pp.; ill. 1950.

With the aim of making this well established textbook of greater help to the medical student, Cowdry has changed the method of presentation followed in previous editions to one in more conformity with the organization of most courses in histology. Beginning with a chapter on the science of histology, its historical development, relation to other sciences, etc., there follows a discussion of cells as vital units and of the elementary tissues of which organs are composed, leading up to the functional organ systems of the body. The correlation between structure and function of cells is stressed throughout. An entirely new feature of the book is a list of questions constituting a final chapter. These questions fall into three categories: first, questions of fact based on the material presented in each chapter; second, constructive questions, the answers to which are tests of the ability to bring together with discrimination information often scattered in several chapters; and third, those questions on histology asked by the National Board of Medical Examiners. More literature references have been offered in this edition than in previous ones. The text is appropriately illustrated and attractively printed.

MARY E. RAWLES

A COLOR ATLAS OF MORPHOLOGIC HEMATOLOGY *with a Guide to Clinical Interpretation.*

By Geneva A. Daland; edited by Thomas Hale Ham; illustrations by Etta Piotti. Harvard University Press, Cambridge, Mass. $5.00. vi + 74 pp.; ill. 1951.

Of all the methods of examination of the blood, from the standpoint of diagnosis and treatment of the patient the study of the blood smear is the most important. This atlas, in that it gives emphasis to this fact and aids in the examination of the blood smear, serves a very useful purpose. It consists of 9 figures which diagrammatically represent the maturation of various types of cells to be found in the blood; 14 plates in color which illustrate the appearance of various types of erythrocytes and leukocytes, as well as blood smears from cases of pernicious anemia, iron deficiency anemia, Cooley's anemia, sickle cell anemia, acute and chronic hemolytic anemia, erythroblastosis fetalis, acute blood loss, chronic lead poisoning, various types of leukemia, and infectious mononucleosis; and a suitable text which gives the essential details to support the illustrations. The drawings are the work of Etta Piotti, who has illustrated the writings of Minot, Castle, and others of the Thorndike Memorial group.

This atlas is intended for students, technicians, and clinicians; it is not meant for the specialist in hematology. This purpose is well served. The text has been carefully prepared and the illustrations are competent.

M. M. WINTROBE

ANIMAL PHYSIOLOGY

PHYSIOLOGY AND ANATOMY. *Sixth Edition.*

By Esther M. Greisheimer. J. B. Lippincott Co., Philadelphia, London, and Montreal. $4.00. xvi + 841 pp.; ill. 1950.

The 6th edition of this very adequate text has been revised and brought up to date with particular attention to the chapters on the muscular system, nervous system, blood, and respiration.

THE EXTERNAL SECRETION OF THE PANCREAS. *Publication No. 45, American Lecture Series. Monograph in American Lectures in Physiology.*

By J. Earl Thomas. Charles C Thomas, Springfield, Illinois. $3.50. x + 149 pp.; ill. 1950.

INTRODUCTION TO THE REGULATION OF BLOOD PRES-

SURE AND HEART RATE. *Publication No. 43, American Lecture Series. Monograph in American Lectures in Physiology.*

By Corneille Heymans. Charles C Thomas, Springfield, Illinois. $2.00. x + 60 pp.; ill. 1950.

FAINTING. *Physiological and Psychological Considerations.*

By George L. Engel. Charles C Thomas, Springfield, Illinois. $2.75. xii + 141 pp. 1950.

SIMMONDS' DISEASE. *Extreme Insufficiency of the Adenohypophysis. Publication No. 34, American Lecture Series. Monograph in American Lectures in Endocrinology.*

By R. F. Farquharson. Charles C Thomas, Springfield, Illinois. $2.00. viii + 93 pp.; ill. 1950.

WATER AND SALT DEPLETION. *Publication No. 32, American Lecture Series. Monograph in American Lectures in Physiology.*

By H. L. Marriott. Charles C Thomas, Springfield, Illinois. $2.00. x + 80 pp.; ill. 1950.

Medical educators will find most of the monographs that compose the American Lecture Series ideally suited for their laboratories, particularly in the preparation of their lectures.

The monograph by Gray deals with The Control of the Respiratory Cycle, Theories of the Control of Ventilation, The Receptor System, Interrelationships between Chemical Stimuli, Respiratory Responses to Chemical Stimuli, The Integrated Regulation of Ventilation, Dissociation and Unsteady States, and Sensitivity and CO_2 Adaptation. This monograph should provide the physician and medical student with a fairly thorough understanding of the operation of such complex regulatory mechanisms, both in health and disease.

Windle has divided his subject into 6 chapters dealing respectively with Fetal-Maternal Relationships, The Blood of the Fetus and of the Newborn, The Fetal Circulation and Changes at Birth, Other Conditions of Intrauterine Respiration, Fetal Respiratory Activity and the Advent of Air-Breathing, and Asphyxia Neonatorum.

Thomas treats his subject from the point of view of the morphology of the pancreas, experimental methods of cannulating the pancreatic duct and making temporary and permanent pancreatic fistulas, the properties, composition, and enzymes of the pancreatic juice and its functions, the type of stimuli for the pancreas, secretin and pancreozymin, functional enervation of the pancreas, and the mechanism of pancreas secretion.

Heymans discusses the Physiological Control of Arterial Blood Pressure, Homeostasis of Arterial Blood Pressure, Efferent Mechanisms of Blood Pressure Homeostasis, Veno-Auricular, Pulmo-Arterial, and Thoracic-Splanchnic Pressoceptive Vascular Areas and Reflex Regulation of Circulation, Vasomotor Centers and Blood Pressure Homeostasis, The Sympathetic Nervous System and Blood Pressure Homeostasis, the Sino-Aortic Nerves and Arterial Hypertension, Chemoreceptors and Blood Pressure Regulation, the Role of the Presso and Chemoceptive Mechanisms on Cerebral Circulation, Coronary Circulation, Muscles in Exercise, and Renal Circulation, Blood Pressure Homeostasis and Circulatory Failure (Shock), and Other Factors Affecting Heart Rate.

The book by Engel is an attempt to describe the mechanism and clinical characteristics of the various types of fainting in the light of modern physiological and psychological knowledge, and should be of especial interest to the physician.

Farquharson's treatise on Simmonds' Disease deals with its initial recognition, pathogenesis and pathology, the clinical picture, diagnosis, treatment, and its relation to other pituitary syndromes.

Marriott in his monograph considers the osmotic difference between water and salt depletion, body water balance, the causes and effects of pure water depletion, body salt balance, the physiological function of salt, the renal relation of extracellular sodium and chloride, the causes and effects of pure salt depletion, and mixed water and salt depletion.

DAVID B. TYLER

ADRENAL CORTEX. *Transactions of the First Conference—November 21–22, 1949.*

Edited by Elaine P. Ralli; Associate Editors: Konrad Bloch and Gregory Pincus. Josiah Macy, Jr. Foundation, New York. $2.00. 189 pp. + 1 pl.; text ill. 1950.

Alice in Adrenaland would like this book. It is mostly conversation, of a kind called discussion. The caterpillar told Alice to reach around the adrenal as far as she could, and to break off a piece in each hand. One side will allay the collagen diseases, the other side will interfere with tissue responses to wounds and infection. The great question is "How?" and "How much?" . . . and all our yesterdays have not turned on the light.

Gertrude Stein would have liked it, too. It is scientific spoken English with little punctuation. A frozen section of time, bristling with hunches.

Formally, the contents are listed as: Relation of Chemical Structure of Adrenal Cortical Hormones to Biological Activity (E. C. Kendall); Regulation of Adrenal Cortical Secretion (Gregory Pincus); Clinical Studies with Cortisone and ACTH (R. F. Loeb); Steroid Metabolism in the Adrenal Cortex (K. Bloch); and Relation of Vitamins to Adrenal Cortical Function (Elaine P. Ralli).

EVELYN HOWARD

SURGICAL PHYSIOLOGY. *Sixth Printing.*

By Joseph Nash. Charles C Thomas, Springfield, Ill.

$6.00. xxii + 496 pp. + 1 folder; ill. 1950 (first printing 1942).

This is not a textbook of physiology. It is an outline of those aspects of physiology which are of especial importance in surgery. It is, however, one of the best organized books I have seen in a long while. Its style is concise and clear and, in some sections, quite conversational. It is very well indexed and contains an adequate bibliography. There are a few errors of fact—but they are minor and in no way detract from the value of the book.

DAVID B. TYLER

PHYSIOLOGY OF THE EYE—*Clinical Application.*

By Francis Heed Adler. C. V. Mosby Co., St. Louis. $12.00. 709 pp. + 2 pl.; text ill. 1950.

This is an authoritative and thorough treatment of the physiology of the eye. The 22 chapters cover all of the major aspects of the visual organ and mechanism. The text is illustrated with over 300 clear figures and drawings. Although primarily oriented toward the clinical interests of medical students and practicing ophthalmologists, the treatment of the subject matter is sufficiently general for visual scientists to find the book of great value. This is unhesitatingly endorsed as the best book on the physiology of the eye since Duke-Elder's monumental *Textbook of Ophthalmology* published in 1934.

A. CHAPANIS

SOME RELATIONS BETWEEN VISION AND AUDITION *Publication No. 71, American Lecture Series. A Monograph in American Lectures in Otolaryngology.*

By J. Donald Harris. Charles C Thomas, Springfield, Ill. $1.50. vi + 56 pp. 1950.

Although at first glance it might seem that the eyes and ears have very little in common, there are in fact many similarities between the two sense organs. Both collect and sort out stimuli impinging on the organism, organizing them in such a way that they become the basis for the meaning we attach to our experiential world. In this little essay, Harris compares the two sense organs in a variety of respects: absolute sensitivity, differential sensitivity, energy integration, growth and decay of sensation, bilateral interaction, and intersensory facilitation. The author makes use of an extensive, though not exhaustive, bibliography to document the various points he discusses. Although the text unfortunately suffers from numerous editorial and grammatical errors, it is generally a highly readable treatise. One could hardly expect a book as brief as this to be profound. Nonetheless, workers in sensory physiology and psychology should find several stimulating ideas in it.

A. CHAPANIS

THE CEREBRAL CIRCULATION IN HEALTH AND DISEASE. *A Monograph in American Lectures in Physiology. Pub. No. 68, American Lecture Series.*

By Carl F. Schmidt. Charles C Thomas, Springfield, Ill. $2.00. vi + 78 pp.; ill. 1950.

Quantitative measurement of cerebral blood flow in man was not possible prior to the introduction of the nitrous oxide absorption method, reported by Kety in 1946. Most of the earlier work was carried out with animals and, because of known anatomical and unpredictable physiological species differences, the knowledge so derived could not be applied to human beings with any certainty. There have been major arguments concerning the degree of extrinsic and intrinsic control of cerebral circulation, the ability of brain vessels to vary their calibre, and the validity of the Munroe Kellie doctrine. More firmly rooted concepts are beginning to emerge as a result of the new methods.

The author ably and briefly reviews the unique anatomy of cerebral vessels, their physiology, including the relationship of their function to cerebral activity, and the present status of knowledge concerning variations of cerebral blood flow in man as a result of drug, nerve, and hormone action, and in certain clinical states. The need for additional quantitative work is stressed at many points. This volume is an authoritative and lucid compilation, especially valuable as a basic reference work.

E. CONVERSE PEIRCE, 2ND

ANIMAL NUTRITION

ESSENTIALS OF NUTRITION. *3rd Edition.*

By Henry C. Sherman and Caroline Sherman Lanford. Macmillan Co., New York. $4.25. x + 454 pp.; ill. 1951.

Like the earlier editions, published in 1940 and 1943, this one presents a condensed, elementary view of the newer aspects of nutrition. It covers the literature through early 1950. Being fundamentally descriptive in nature, without formulas and chemical symbols, the book is suitable for students with little or no previous science training.

Among the subjects covered are: (1) the energy aspects of nutrition, (2) the proteins and their amino acids, (3) the mineral elements, and (4) the vitamins. In the chapters on vitamins, the newer additions to the recognized B vitamins including folic acid and vitamin B_{12}, are discussed.

Interesting accounts are given of early observations leading to the recognition of dietary diseases, thus helping the new student to appreciate the meager knowledge of nutrition at the turn of the century. The historical background also gives added stress to the significance of the advances in nutrition in recent years.

R. VAN REEN

ANIMAL NUTRITION. *Third Edition.*

By Leonard A. Maynard. McGraw-Hill Book Company, New York, Toronto, London. $6.50. x + 474 pp.; text ill. 1951.

The contents of this book are divided into four parts: General Bases of Nutrition; The Nutrients and Their Metabolism; The Measurement of Body Needs and Feed Values; and Nutritive Requirements for Body Processes and Productive Functions. The author adequately covers both the principles and techniques useful in animal nutrition. Since the publications in the field of nutrition have been voluminous during the past years, it is a difficult task to select critically from the literature only the most important information, and to keep the book approximately to the size of the earlier editions. To meet this challenge well, the author found it necessary to delete some topics from the earlier edition and to add newer or revised material, such as discussions of the use of isotopes in studies of metabolism, vitamin B_{12}, etc. This book should be a very useful textbook.

BACON F. CHOW

PROTEIN AND AMINO ACID REQUIREMENTS OF MAMMALS.

Edited by Anthony A. Albanese. Academic Press, New York. $4.00. x + 155 pp.; ill. 1950.

In the Foreword it is stated that the purpose of this volume is to describe the techniques currently employed in the determination of the protein and amino acid requirements of various mammals, and of man in particular. The 5 chapters are: Some Species and Age Differences in Amino Acid Requirements (H. H. Mitchell); Methods of Measuring the Nutritive Value of Protein Hydrolysates and Amino Acid Mixtures—the Rat Depletion Method (Douglas V. Frost); The Laboratory Evaluation of Amino Acid Mixtures and Protein Hydrolysates (Robert H. Silber and Curt C. Porter); Dietary Proteins and Synthesis of Tissue Proteins (Bacon F. Chow); and The Protein and Amino Acid Requirements of Man (Anthony A. Albanese).

This book is highly recommended to those who desire to gain a clear conception of the state of progress in elucidating the perplexing problems involved in protein metabolism. There is uniqueness in the metabolic pattern of each type of body structure, such as digestive glands, endocrine organs, liver, or muscle. There is endless variety in the structure and composition of proteins in food proteins. Year by year improvements have been devised in experimental methods for revealing what takes place in protein nutrition in growth, maintenance, fasting, and in the chemical processes involved in the synthesis of specific tissue proteins from amino acids absorbed from the digestive tract. Every biochemist, physiologist, and medical investigator will gain much by careful study of this volume.

E. V. MCCOLLUM

NUTRITION AND CLIMATIC STRESS *with Particular Reference to Man.*

By H. H. Mitchell and Marjorie Edman. Charles C Thomas; Springfield, Ill. $6.75. xii + 234 pp.; ill. 1951.

Over the period 1946–1948 the authors prepared a manuscript report on this subject for the Quartermaster Food and Container Institute. This was issued in 1949 in lithographed form. The text of the present publication differs very little from that of either of the previous formats. Unlike most of the bibliographic reports prepared for the armed services, it has literary form, though a somewhat staccato one, and gives the authors' assessment of the findings reported in the approximately 800 sources quoted. This assessment naturally occupies more space and is more cogent when it deals with aspects on which the authors have personally carried out investigations. One cannot help regretting that the same attention and penetration was not sustained throughout. (Data obtained by the reviewer in the combat zone of New Guinea were quoted (p. 48) as coming from Southern Queensland, some 1500 miles to the south.)

Stressful environments are divided into three categories—cold, hot, and altitude. For each category the effect of the environment upon nutritional requirements, and the effect of diet upon tolerance to the environment are separately considered. Animal experiments are carefully distinguished from human experiments and observations, and the point is emphatically made that evidence should be transferred from animal experiments to human affairs only with great caution. One tends to be impressed rather more by the numerous instances in which no clear relationship can be established between diet and climatic stress, than by the importance of dietary control. In an age which seeks environmental determinants of human behavior rather too avidly, this conservatism should have a corrective effect. Where the evidence is inconclusive, as it frequently is, the authors say, not that there is no connection, but that action based on a suppositious connection should be withheld.

DOUGLAS H. K. LEE

NUTRITION IN HEALTH AND DISEASE. *Eleventh Edition, revised and reset.*

By Lenna F. Cooper, Edith M. Barber, Helen S. Mitchell, and Henderika J. Rynbergen. J. B. Lippincott Co., Philadelphia, London, and Montreal. $4.00. xviii + 744 pp. + 4 pl.; text ill. 1950.

The 11th edition of this excellent textbook maintains the same high standards of scientific accuracy and pedagogic soundness exhibited by the earlier editions. Thoroughly revised and brought up to date, the volume reflects the latest developments in both the principles and practices of nutrition as related to health and disease.

As in the earlier editions, the subject materials are presented under four general topic headings, namely: Principles of Nutrition; Diet in Disease; Food Selection and Preparation; and Tabular Material and Special Tests. Part One covers such general principles of nutrition as those relating to bodily requirements for the various food elements, the food sources which supply these requirements, and the importance of the balanced diet in the maintenance of sound health. The selection and preparation of foods best suited for patients during illness and convalescence from a variety of diseases is emphasized in Part Two. Part Three is a compendium of diets, menus, and recipes designed for use in the preparation of a wide variety of nutritionally balanced meals. The tables in Part Four are concerned largely with the nutritive values of various food products.

Although the volume is designed primarily for use as a textbook in nurses' training, it will also serve as an excellent source of reference for graduate nurses, public health nurses, dietitians, and general students or teachers of home economics. The work is well illustrated and carries a selected list of references, a glossary, and a detailed index. The present edition will undoubtedly insure the continued popularity of the book as a favorite in its field.

B. Aubrey Schneider

Nutrition Fronts in Public Health. *Nutrition Symposium Series, Number 3. Proceedings of the Nutrition Symposium held at Yale University, November 10, 1950.*

By Icie G. Macy, Benjamin H. Ershoff, Clive M. McCay, Ernst Simonson, Howard A. Schneider, and Leo T. Samuels. National Vitamin Foundation, New York. $1.50 (paper). vi + 168 pp. 1951.

This book is the final volume resulting from the comprehensive study by Icie G. Macy and the staff of the Research Laboratory of the Children's Fund of Michigan concerning the growth of healthy children. Volume I of this series detailed the methods used in the studies; Volume II contained the original data. The compilation of calculated data presented in this volume includes the average daily values for intake, absorption, and retention of carbohydrate, fat, nitrogen, phosphorus, chlorine, sulfur, calcium, magnesium, sodium, potassium, and the excess of positive or negative minerals. The data are converted to the basis of kilogram of body weight, centimeters of body length, and square meters of body surface. Means and standard deviations of the data are also presented.

R. Van Reen

GENERAL PHYSIOLOGY

Biological Antagonism. *The Theory of Biological Relativity.*

By Gustav J. Martin. The Blakiston Company, Philadelphia. $8.50. viii + 516 pp.; ill. 1951.

This volume covers most of the rather extensive literature of the past 10 years on the general subject of metabolite antagonists. Its value will be as a source of references rather than as a critical treatise. The theory of biological relativity is merely a re-statement of known concepts. In this discussion the author is entirely unconvincing in attempting further to elucidate Schrödinger's *What Is Life?*

Richard O. Roblin, Jr.

BIOCHEMISTRY

Les Isotopes Radioactifs en Médecine et en Biologie.

By Maurice Tubiana; preface by Pr L. Bugnard. Masson et Cie., Paris. 1800 fr. (paper). x + 391 pp.; ill. 1950.

This book gives another review of a field in which there are now a number of books in English. Its particular advantages are that it is written in French for French scientists, that it begins in a quite elementary way and discusses the various subjects quite simply, and that it provides a rather broad coverage of the field. Like most books written today on isotopes, the scope of the book does not include the subject of stable isotopes. This is unfortunate, since for the elements nitrogen and oxygen only stable isotopes are available, for hydrogen deuterium has had much wider application than tritium, and for carbon the stable isotope has a number of unique advantages. The field of stable isotope applications is not known very widely, although the potentialities of those methods are tremendous.

Like many such books, this one can hardly be used as an orientation in the field, since it contains too much to digest. On the other hand, it can hardly serve as a basic reference in any part of the field, since it is not complete or rigorous enough.

Frederick W. Barnes, Jr.

Radioactivity Applied to Chemistry.

Edited by Arthur C. Wahl and Norman A. Bonner. John Wiley & Sons, New York; Chapman & Hall, London. $7.50. xvi + 604 pp. 1951.

As the title implies, this book is intended primarily for chemists. The subjects covered are chiefly of a physical inorganic nature, although some space is devoted to the mechanisms of organic reactions. No mention is made of such subjects as tracer techniques in studying photosynthesis or other biological reactions. Therefore, the book has little to offer to biologists not actively engaged in radiochemical work.

For those who are carrying on tracer studies of any sort, however, the book is highly recommended because

of the wealth of information contained in the extensive tables and in the textual material. Thirty-eight pages of bibliography cover the chemical literature into 1949, and in the case of domestic journals through December, 1949. Of particular interest are the tables summarizing tracer methods applied to kinetics, analysis by tracer methods, hot atom chemistry, and self-diffusion coefficients. These and other summary tables fill approximately 200 pages. The discussion of selected experiments in the body of the text is presented in a clear, straightforward fashion. The physical make-up of the book is excellent, and no typographical errors were noted.

J. W. GRYDER

PHYSICAL CHEMISTRY FOR PREMEDICAL STUDENTS. *Second Edition. International Chemical Series.*

By John Page Amsden. McGraw-Hill Book Co., New York, Toronto, and London. $4.25. xii + 317 pp.; ill. 1950.

Numerous minor changes and two more extensive alterations mark the 2nd edition of this textbook (*Q.R.B.*, 21: 281, 1946). The major changes are (1) an alteration of the approach to discussion of the proteolytic equilibrium; and (2) a new chapter on thermodynamics. The latter goes far to meet the major criticism directed at the book when it first appeared, namely, that it did not give an adequate treatment of certain topics in physical chemistry which are of primary importance to premedical students. However, the thermodynamics of catalyzed processes and the effect of temperature on the speed of reactions are still given short shrift. A bibliography is still needed to guide the student to such topics.

BENTLEY GLASS

ANNUAL REVIEW OF PHYSICAL CHEMISTRY. *Vol. I. Edited by G. K. Rollefson, with R. E. Powell. Annual Reviews, Stanford, Cal.* $6.00. viii + 382 pp. 1950.

This contribution to two-dimensional order in the field of physical chemistry is a welcome addition to the family of annual reviews. The editors state that it will be their policy to present reviews of various topics at more or less regular intervals which will be determined by the activity in the fields. About 15 years have passed since the appearance of any publication giving a general review or survey of thermochemistry and thermodynamics, consequently the article on these subjects by F. D. Rossini, D. D. Wagman, W. H. Evans, and J. E. Prosen is little more than a collection of references as a basis for subsequent reviews. The reviews proper are: Heterogeneous Equilibria and Phase Diagrams (L. Brewer); Solutions of Electrolytes (A. R. Gordon); Solutions of Non-electrolytes (J. E. Hildebrand and R. L. Scott); Radioactivity and Nuclear Theory (W. F. Libby); Radiation Chemistry (M. Burton); Theories of Valence (H. C. Longuet-Higgins and G. W. Wheland); Spectroscopy (B. L. Crawford, Jr. and D. E. Mann); Statistical Mechanical Theory of Crystalline and Liquid State (J. E. Mayer); Experimental Molecular Structure (J. Y. Beach); X-ray Structural Crystallography (J. L. Hoard and S. Geller); Chemical Kinetics (F. Daniels); Contact Catalysis and Surface Chemistry (H. S. Taylor); Photochemistry (W. A. Noyes, Jr., and A. J. C. Nicholson); Polarography (H. A. Laitinen); Effect of Oxygen on the Physical and Chemical Properties of Polymers (H. F. Mark and R. B. Mesrobian); Copolymerization (H. F. Mark and T. Alfrey, Jr.); and Colloid Chemistry—Exclusive of High Polymers (H. B. Weiser).

EVELYN HOWARD

PRINCIPLES OF IONIC ORGANIC REACTIONS.

By Elliot R. Alexander. John Wiley & Sons, New York. $5.50. viii + 318 pp. 1950.

A somewhat descriptive account of the *mechanisms* of a number of important organic chemical reactions, of interest chiefly to the graduate student in chemistry and to others who need a review of organic theory. There are useful references to the literature.

SIMPLIFIED CHEMISTRY EXPERIMENTS.

By Armand Joseph Courchaine; edited by M. Cordelia Cowan; illustrated by Richard Albany. G. P. Putnam's Sons, New York. $2.80 (paper). xxii + 234 pp.; ill. 1950.

This manual describes elementary chemistry experiments that may be used in chemistry courses with time allotments up to 120 hours.

COLLOID CHEMISTRY *Theoretical and Applied. Volume VII. Theory and Methods—Biology and Medicine—Technological Applications.*

By Selected International Contributors; collected and edited by Jerome Alexander. Reinhold Publishing Corporation, New York. $15.00. xii + 736 pp.; ill. 1950.

The range of the articles in this weighty compendium is that of all physical and biological science, with a huge segment of technology thrown in: everything from nuclear reactions to dental caries; from aerosols to wood. Most of the contributions are individually authoritative and valuable as references, but it is also true that most of these subjects have been reviewed recently by the same authors or by others in review journals or in books more homogeneous in content than the present volume.

Articles of particular biological interest are as follows: Paper Chromatography of Amino Acids (R. J. Block & H. A. Sober); Successive Levels of Material Structure (J. Alexander); Surface Chemistry and Biology (J. Alexander); Complexes of Ions with Proteins (I. M. Klotz); Catalysts and Neocatalysts in Biology and Medicine (J. Alexander); Adaptive Formation of Enzymes and Influence of Nitrogen Nutrition on the Enzymatic Activity of Cells (A. I. Virtanen); Some Aspects of the Interaction of Drugs and Enzymes (F. Bernheim); Genes and Biochemical Reactions (D. M. Bonner); Chemical Problems in the Genetics of Disease Processes (J. W. Gowen); Antibiotic Colloidal Electrolytes (E. A. Hauser); The Tubercle Bacillus (L. Grigoraki); Blood Groups, with Special Reference to the Rh-Hr Factors (A. S. Wiener); Dental Caries (L. S. Fosdick); Wood—an Example of a Colloid System (H. Tarkow & A. J. Stamm).

Excellent as many of these articles are, it seems impossible to imagine any one reader with needs so vast as to warrant the purchase of a work so expensive and so heterogeneous as this. It will find its place in a few libraries, and that is probably all. At a cost of 2 cents per page, it cannot be recommended to the individual buyer. Had the editor separated the three parts into so many volumes, the usefulness of each would have been enhanced, although even so the heterogeneity of the contents would remain great. In these days of high costs of publication and of so numerous series of symposia, "recent advances," and "annual reviews," everything possible should be done to ensure (a) that only really superior reviews and general articles are published, and (b) that these are collected in the form most economical and most useful to the potential users.

BENTLEY GLASS

PLANT BIOCHEMISTRY.
By James Bonner. Academic Press, New York. $6.80. xviii + 537 pp.; ill. 1950.

The rapid development during the last decade of our knowledge of biochemical processes in the higher plants, knowledge contributed in large part by animal and microbial biochemists, is surely and inevitably leading to the establishment of plant biochemistry as a separate field, no longer a subdivision of plant physiology but actually quite independent of it.

The present book represents in a sense the "coming of age" of plant biochemistry in the United States. The volume is of very high caliber, in its wide coverage of the literature and integration of material. These features, together with a simple, straightforward style of presentation, provide the discipline of plant biochemistry with a very adequate text, certainly equal to, if not superior to, any published thus far in plant or agricultural biochemistry. The book has been designed as a textbook for advanced undergraduate and early graduate courses and requires a reasonable knowledge of organic chemistry and to a lesser extent of physical chemistry.

Those matters which especially concern plants, such as the metabolism of starch and of the cell wall, photosynthesis, plant growth, and secondary plant products, occupy approximately half the volume, and are well supplied with illustrations and structural formula. Surprisingly enough, the topic of photosynthesis is given the smallest treatment of the above sections. The sections dealing with plant respiration and nitrogen metabolism are approached for the most part from the desirable comparative biochemical viewpoint. In these chapters the author uses the extensive biochemical information regarding microorganisms and animals to serve as a fundamental pattern on which the facts of plant biochemistry are superimposed.

Although the facts are set forth in a well-ordered series of views, the author has chosen to present his material in the traditional factual manner of most textbooks. The failure to highlight the important experimental approaches that have yielded the significant facts, as well as the insufficiency of original experimental data given in the form of curves and tables, and which are so important in stimulating and developing a student's critical sense and experimental approach, constitute a major detraction. Other failings are the absence of a section on mineral nutrition and metabolism, plus the minor omissions of such enzymes as carbonic anhydrase, formic dehydrogenase, glycolic dehydrogenase (oxidase), and dehydroascorbic reductase.

Nevertheless, the over-all effect has been to create a well-organized and integrated first-rate work on plant metabolism which includes a richly annotated bibliography. It should go a long way toward advancing the status of plant biochemistry in this country.

ALVIN NASON

ADVANCES IN CARBOHYDRATE CHEMISTRY. *Volume 5. Edited by Claude S. Hudson and Sidney M. Cantor. Associate Editors for the British Isles: Stanley Peat and Maurice Stacey. Academic Press, New York.* $6.80. xii + 322 pp. 1950.

Volume 5 of the Advances in Carbohydrate Chemistry maintains the high standard of excellence found in the preceding volumes. These reviews, while bringing the reader up to date with the literature, also present the opinions of investigators active in the field of carbohydrate chemistry and are valuable adjuncts to the chemists and biologists looking for new methods and interpretations in this complex field. The present volume contains 10 reviews. The Commercial Production of Crystalline Dextrose, by G. R. Dean and J. B. Gottfried, gives a detailed account of the technology involved in modern dextrose production. Three of the reviews are of interest primarily to the chemist working with methods of synthesis or structure proof: Applica-

tions in the Carbohydrate Field of Reductive Desulfurization by Raney Nickel (Hewitt G. Fletcher, Jr. and Nelson K. Richtmyer), The Methyl Esters of D-Glucose (E. J. Bourne), and Anhydrides of the Pentitols and Hexitols (L. F. Wiggins). The review on The Relative Crystallinity of Celluloses, by R. F. Nikerson, contains much information of interest to the plant physiologist and pathologist. It would appear that a study of the relative crystallinity of the fibers of plants considered resistant or susceptible to certain pathogens might yield valuable information. Xylan, by Roy L. Whistler, reviews a domain of plant chemistry which, in so far as the biological and physiological aspects are concerned, is but poorly understood. Another aspect of the nature of the cell wall of plants is dealt with by Z. I. Kertesz and R. J. McColloch in the review of Enzymes Acting on Pectic Substances. These enzymes are valuable tools in the elucidation of the chemical nature of pectic substances. Mary L. Caldwell and Mildred Adams, in the Action of Certain Alpha Amylases, consider these enzymes in the light of their usefulness in the characterization of starch and glycogen. The last two reviews may be considered as required reading for the biochemist. The Enzymatic Synthesis of Sucrose and Other Disaccharides, by W. Z. Hassid and M. Doudoroff, reviews the biological and chemical synthesis of these basically important compounds. The review by Alfred Gottschalk on the Principles Underlying Enzyme Specificity in the Domain of Carbohydrates gives a clear, concise statement of the problem of the specificity of enzymes as related to the configurational and substitutional changes in the sugar molecule.

G. R. NOGGLE

BIOCHEMIE UND PHYSIOLOGIE DER SEKUNDÄREN PFLANZENSTOFFE. *Lehrbuch der Pflanzenphysiologie, Erster Band, Zweiter Teil.*

By Karl Paech. Springer-Verlag: Berlin, Göttingen, Heidelberg. DM 24.00 (paper); DM 26.70 (cloth). x + 268 pp.; ill. 1950.

It is by now commonplace among biochemists to regard the fundamental metabolic processes of different kinds of organisms as proceeding along similar, if not identical, pathways. Thus glycolysis in muscle is apparently not significantly different from that which occurs in yeast; the organic acid cycles of the *Avena* coleoptile are probably the same as those taking place in pigeon breast muscle; the ornithine-citrulline-arginine cycle of mammals is closely paralleled by that of *Neurospora*. These processes of general significance have been intensively studied and are well known to biochemists. Less familiar to biochemists as a group are the biogenesis, chemical nature, and metabolic transformations of numerous substances which occur exclusively in higher plants. This volume by Paech is a brief exposition of this relatively esoteric aspect of biochemistry.

By "secondary plant materials" the author means all those substances formed from the basic materials of plants, that is, the carbohydrates, amino acids, and simpler substances. He starts with a brief consideration of some general aspects of metabolism, then passes to a detailed discussion of specific classes of compounds found in plants. Among the topics covered are: organic acids and their transformations, fat metabolism (including waxes, suberin, and cutin), terpene compounds (including carotenoids, rubber, resins, essential oils, and steroid substances), nitrogen-free aromatic compounds (including phenols, tannins, lignins, condensed-ring systems, and flavones), and nitrogen-containing secondary plant materials (including amines, alkaloids, purines, cyanogenetic compounds, and mustard oils). It is obviously a task of major proportions to condense the literature of so vast a field into so small a volume. The author must be congratulated for the fine scale of values he has displayed in making this condensation.

Technically, this volume is part of a textbook of plant physiology, other portions of which have previously appeared. It is therefore to be expected that more than the usual amount of attention should be paid to such matters as the biogenesis and physiological significance of the compounds discussed. Unfortunately, our comparative ignorance of such subjects forces the author to rather undue amounts of paper chemistry and speculation. It is to be hoped that biochemists will appreciate these deficiencies and will be stimulated to supply some of the missing data. In dealing with certain classes of compounds (for example, rubber and purines), the author has failed to present recent significant data. There are of course occasional errors of fact and of citation, but these are rare enough so as not to detract appreciably from the book.

This book is a definite improvement over available treatises on this phase of plant biochemistry. When supplemented by an adequate treatment of the enzymes of plants, simple carbohydrates, polysaccharides, amino acids, proteins, and mineral metabolism, it will serve as a valuable reference work for the plant biochemist. It is not, however, to be recommended in its present form as a complete textbook of plant biochemistry.

ARTHUR W. GALSTON

TEXTBOOK OF BIOCHEMISTRY. *Fifth Edition.*

By Benjamin Harrow. W. B. Saunders Company, Philadelphia and London. $6.00. x + 609 pp.; ill. 1950.

This first-rate and well-known textbook has been considerably revised to include much new material without substantially changing the number of pages. The style and format of previous editions has been maintained.

DAVID B. TYLER

ONE FAMILY: VITAMINS, ENZYMES, HORMONES.
By Benjamin Harrow. Burgess Publishing Company, Minneapolis. $2.00 (paper). v + 115 pp. 1950.

In this little book Benjamin Harrow demonstrates once more his gift for popularizing the results of biochemical research. A very readable, and in many ways technically satisfying account is achieved of the work which has yielded our recently unified view of vitamins, enzymes, and hormones. Anecdotes are interspersed amongst the technical details with the practised ease of the accomplished lecturer.

The net result must be very close to the aim of the author. He has provided a pleasant and painless mechanism for the acquisition of insight into an important branch of modern biochemical research.

S. SPIEGELMAN

ENZYMES, GROWTH AND CANCER. *Publication No. 75 American Lecture Series. A Monograph in American Lectures in Physiology.*
By Van R. Potter. Charles C Thomas, Springfield, Ill. $1.85. 64 pp. 1950.

V. R. Potter, one of the very active workers in the field of enzymology, has prepared a concise and clearly written exposition of what is known regarding the mechanisms influencing enzyme activity. The monograph is based, for the most part, on a series of lectures given while he was Commonwealth Visiting Professor of Biochemistry at the University of Louisville in March, 1947. It can be highly recommended as a teaching guide covering this subject for biology and biochemistry courses at the college level.

DAVID B. TYLER

SEROLOGY WITH LIPID ANTIGEN, *with Special Reference to Kahn and Universal Reactions.*
By Reuben L. Kahn. The Williams & Wilkins Co., Baltimore. $6.00. xii + 327 pp.; ill. 1950.

Kahn describes in this book his method for the serological diagnosis of syphilis and reports on its history and its continuous development and improvement. The great responsibility of all those who carry out such tests is evident when one considers that erroneous positive reactions may give rise to social tragedies. In view of this danger it is bewildering to see that the outcome of the serological test depends on minute technical details such as the "ripening" of the antigen solution or on the mode of shaking. It is astonishing that the tests are adequate, in spite of these difficulties. Biologists will be particularly interested in the fact that the serological lipid reaction can be made positive in all human and animal sera by using appropriate physical chemical conditions, and that different animals show a different pattern of reaction. Unfortunately, not much is known about the nature of the reactants or the mechanism of this important reaction. The book contains a wealth of material and will be indispensable to all persons engaged in the serum diagnosis of syphilis.

FELIX HAUROWITZ

MICROBIOLOGY

MICROBIOLOGY—*General and Applied.*
By William Bowen Sarles, William Carroll Frazier, Joe Bransford Wilson, and Stanley Glenn Knight. Harper & Bros., New York. $4.50. xii + 493 pp.; ill. 1951.

This survey of the field of general microbiology gives greater attention to the application of microbiology to agriculture and industry and less attention to the disease-producing activities of microbes than is found in most other textbooks. Otherwise the selection and organization of the material is orthodox, although the outline of the History of Bacteriology is placed at the end of the book. In common with most other textbooks, the authors introduce molds, yeast, viruses, protozoa, and higher bacteria to the student before he is firmly rooted in the concepts and terminology of bacteriology. As a result, he encounters the specialized sections on the physiology of molds, on the cultural and physiological characteristics of yeast, or on the classification of protozoa, before the general treatment of these subjects as applied to bacteria. It is disappointing to find that the selection of diseases, which are treated in an encyclopedic manner, is still on the basis of national rather than of global importance, since we feel that the students who use this book will not be subject to any such limitation.

Students will like this book. It will facilitate the assimilation of the factual material which is the backbone of this science. Concise statements, with key words accentuated by upper case and grouped into short paragraphs under outline-form headings, characterize the style. The 70 illustrations are refreshingly new and have teaching value; the expected woodcut of van Leeuwenhoek and the daguerreotype of Pasteur happily have been omitted. References are sharply curtailed and with few exceptions are to major presentations.

Some instructors, however, will not like this book. Those who feel that the historical development and evolution of a science is the effective method for inculcating the feeling for science at the elementary level, and who consequently would start with biological nitrogen fixation and the triumphs and errors of Boussingault, Ville, and Liebig, will be disappointed by this typical introduction:

> "Certain bacteria and at least one kind of blue-green algae growing independently are able to fix atmospheric nitrogen in their cells."

Those who rely on the analysis of the results of experiments to stress the place of imagination and logic in

science will find that treatment almost absent in this textbook. Those who would emphasize that biology is a growing science in which our present knowledge represents only temporary and initial steps will regard the present work as overly precise in its attitude. Those who feel that the instructor has no other resources than the textbook (and from examining others, this is no negligible class) will be disappointed by the many omissions. And all of us will resent the fact that our pet subjects have been treated in a more superficial manner than they deserve. Yet this may prove to be the strength of this book and may assure it a wide usefulness, since it will permit the instructor from the lecture platform and in the laboratory to follow his inclinations in the development of this discipline, secure in the knowledge that the student has on his study desk a usable, abridged compendium of microbiological facts.

ORVILLE AND MARGARET WYSS

MANUAL OF BACTERIAL PLANT PATHOGENS. *Second Edition. Annales Cryptogamici et Phytopathologici, Volume X.*

By Charlotte Elliott. Chronica Botanica, Waltham, Mass.; Stechert Hafner, New York. $6.00. xii + 186 pp.; ill. 1951.

The original edition of Elliott's *Manual* was hailed as the first "adequately complete record in the English language of the bacteria causing disease in plants." The revised edition brings that record up to date and possesses several major improvements over the original edition. One of them is the adoption in general of the nomenclature used by W. H. Burkholder in the Sixth Edition of Bergey's *Manual of Determinative Bacteriology*. While this nomenclature has not been universally accepted, it is widely used on this continent, and its adoption will make Elliott's manual more useful to those who may not be entirely familiar with the older classifications of bacterial phytopathogens.

The book consists of Part I, Bacterial Plant Pathogens, and Part II, Bacterial Species Reported in the Literature from Plants but Not Recognized as Valid Plant Pathogens. In both sections condensed information is given by organism within generic groups, the genera being in alphabetical arrangement and the species alphabetical within the genera. For each pathogen is given the synonymy with authorities, the chief cultural characters, the symptoms of the disease, the hosts, geographical distribution, control, and literature references. These references, continuing into the year 1948, constitute a valuable source of material for laboratories that do not maintain adequate reference files. No attempt is made to supply textbook knowledge such as is included in Dowson's *Manual of Bacterial Plant Diseases*.

The well-arranged index of genera and species in the revised edition is a welcome improvement. In view of the past instability of generic names and the relative stability of the specific epithets of bacterial phytopathogens, the author lost nothing while gaining accessibility by omitting to group the species within genera.

On the other hand, in the index of plant hosts, she wisely retained the grouping of species within genera.

Charlotte Elliott, one-time associate of the late Erwin F. Smith, has performed a praiseworthy revision and greatly enhanced the value of her manual.

W. A. F. HAGBORG

HEALTH AND DISEASE

TEXTBOOK OF HEALTHFUL LIVING. *New Fourth Edition.*

By Harold S. Diehl. McGraw-Hill Book Co., New York, Toronto, and London. $4.00. xxii + 776 pp.; ill. 1950.

This is a sound, thorough, factual, and comprehensive textbook on personal and public health. The material is well organized and lucidly presented, and manages to be thought-provoking, too. The breadth of concept of the book may be illustrated by listing some of the subjects presented: government and public health; voluntary health services; accidents; dietary dangers; conservation of vision; sanitation; communicable diseases; mental health; industrial hygiene; and care of the nose, throat, and ears. The chapters on sex life and parenthood are particularly good. However, there is little on international health problems, and the discussion of housing could be expanded.

In addition to the freely illustrated text, a great deal of useful material and data has been incorporated in the volume. There are ample references for supplementary reading, and there is fine glossary. Appendices give weight tables, a table of the nutritional value of foods, a personal health record form, and a reprint of the seventh edition of the official report of the American Public Health Association, *The Control of Communicable Diseases*. Correlated with the text are 5 motion pictures and film strips, available from the publisher.

This book can be recommended as excellent to put in the hands of young—and no longer young—adults for study, for reference, or just for reading. The apt and extensive quotations add flavor and serve to lend continuity with the classics and the history of science. Finally, to quote the author's dedication, the book really is for "those who prefer facts to fads, sanity to superstition, understanding to belief."

HARRIETTE VERA

ELEMENTS OF HEALTHFUL LIVING. *New Second Edition.*

By Harold S. Diehl. McGraw-Hill Book Co.: New York, Toronto, and London. $3.00. x + 330 pp.; ill. 1950.

The abridgment of the excellent *Textbook of Healthful Living* lacks the glossary, food tables, and the reprinting of *The Control of Communicable Diseases*. Much of the text has been deleted. Although what remains is a worthwhile book, with comparatively little more expense the owner can have very much more by buying the full text.

HARRIETTE VERA

NEW DISCOVERIES IN MEDICINE. *Their Effect on the Public Health.*

By Paul R. Hawley. Columbia University Press, New York. $2.50. x + 134 pp.; ill. 1950.

The Bampton lectures delivered at Columbia University in 1949 are popular presentations of four subjects: Our Fabulous Blood; Modern Surgery of the Heart and Lungs; Modern Prevention and Treatment of Mental Disease; and The Socio-Economic Aspects of Medical Care. Almost half of the book is on blood. This account is somewhat labored, with perhaps undue emphasis on historical background.

The sections on thoracic surgery and mental health are brief and exciting. The jacket promises photographs of operations—they were apparently shown at the lectures, but were not included in the book. The public health implications of thoracic surgery and prefrontal lobotomy are comparatively slight, because of an insufficient number of cases and insufficient lapse of time for the evaluation of results. The last section summarizes arguments for voluntary health insurance, and is both dynamic and persuasive. Such "new" discoveries as the antibiotics and the use of radio-active isotopes, which have very broad public health applications, were not discussed.

HARRIETTE D. VERA

THE PATHOGENESIS AND PATHOLOGY OF VIRAL DISEASES. *Sympos. Sect. Microbiol., N. Y. Acad. Med.—No. 3 (December 14 and 15, 1948).*

Edited by John G. Kidd. Columbia University Press, New York. $5.00. x + 235 pp.; ill. 1950.

In May of 1947, the Section on Microbiology of the New York Academy of Medicine was organized, the first section of the Academy to be devoted to basic medical sciences. This volume consists of papers presented at the third Symposium of that Section.

Since much of what we know about viruses, and many of our methods of studying them, are based upon their effects in a susceptible host, the pathology and pathogenesis of viral infections have always been an integral part of their investigation. The physical and chemical approach to virus study has been stressed in recent years, and it may be salutary to return to the "soil."

The publication represents the contributions of 14 scientists, well qualified by extensive study in the virus field. The book can best be described by indicating the contents of the several chapters. After a short introduction by John G. Kidd, there are 5 chapters having to do with general fields of study or interest. A chapter on the spread of viruses from infected to susceptible hosts is contributed by Richard E. Shope, who uses representative viral diseases of animals to illustrate various mechanisms of transmission. C. John Buddingh has contributed an interesting lecture on the culture and effects of viruses in chick embryo cells. He stresses particularly the variation in susceptibility of the different parts of the embryo during the course of its development, and the physiologic basis for this variation. Although there is little direct evidence that these variations depend upon the level of metabolism of the various tissues at different times, there is considerable indirect evidence that this is true. Gilbert Dalldorf, in a discussion of the relationships of viruses and cells, describes the recently intensively studied phenomenon of the interaction of influenza virus and erythrocytes, and discusses and analyzes the interference phenomenon in some detail. In a chapter on the activation of viruses by adsorption cofactors, Thomas F. Anderson points out the valuable contributions to the general field of virology that have been made in recent years by extensive researches in the mechanism of action of bacteriophage. It is pointed out specifically that the evidence from such studies suggests that cofactor mechanisms may play a role in the behavior of animal viruses, knowledge which in turn provides a *rationale* and a method of approach to the problem of chemotherapy of these diseases. Ralph W. G. Wyckoff has contributed a chapter on the electron microscope study of virus growth, in which the phenomenon of lysis of bacteria by bacteriophage is described in some detail.

The remaining chapters discuss specific viral diseases or groups of viral diseases. T. F. McNair Scott and his colleagues have contributed a chapter on the cutaneous lesions of variola, vaccinia, herpes simplex, herpes zoster, and varicella. Frank Fenner extends this discussion in his chapter on mouse pox (infectious ectromelia). A chapter on cardiac lesions produced by viruses is contributed by John N. Pearce, and contains a discussion in considerable detail of experiments in which a number of viruses were induced to provoke lesions in the rabbit heart, and the various physiologic factors influencing susceptibility of the cardiac tissue. J. E. Ash has contributed a short chapter on the pathology of yellow fever, containing an informative collection of photomicrographs. Thomas P. Magill's chapter on the reaction of the cells of the respiratory tract to virus infections discusses mainly infection with influenza or psittacosis virus. A chapter on proliferative lesions caused by viruses and virus-like agents, by John C. Kidd, is mainly concerned with studies on the carcinogenic effect of the rabbit papilloma virus and the proliferative lesions caused by certain virus-like agents.

R. D. Lillie discusses the pathology of lymphocytic choriomeningitis, and Abner Wolf describes the pathology of certain viral encephalitides. A separate chapter by Howard A. Howe is devoted to the nature and pathogenesis of neuronal changes in poliomyelitis.

Most of the chapters are well illustrated with photomicrographs of histologic sections, electron micrographs, or by charts or graphs. Each chapter is adequately documented with a list of references. There is no index. This volume will have considerable interest for a wide range of readers and will be informative and stimulating to all students of infectious disease.

F. B. GORDON

BACTERIAL INFECTION *with Special Reference to Dental Practice. Fourth Edition.*

By J. L. T. Appleton. Lea & Febiger, Philadelphia. $10.00. 644 pp. + 5 pl.; text ill. 1950.

The writer has had two purposes in writing this textbook: to give a comprehensive concept of infection, and to present the subject of oral bacteriology. The book is in three parts: an Introduction to Bacteriology; a discussion of Infection and Immunity; and a summary of information concerning Infections Peculiar to, or Associated with, the Oral Cavity, or having Oral Manifestations. The final chapter is on the bacteriologic aspects of oral hygiene.

This important book—on an important subject—is designed primarily for dental students and dentists, but is of interest to physicians, teachers of personal hygiene, and others concerned with oral bacteriology. It should also be useful for reference in public libraries.

HARRIETTE D. VERA

PATHOLOGIC PHYSIOLOGY: *Mechanisms of Disease.*

Edited by William A. Sodeman. W. B. Saunders Co., Philadelphia and London. $11.50. xxiv + 808 pp.; ill. 1950.

This volume, prepared by 25 authors, should be of value to the medical student and physician in understanding the clinical signs of disease in terms of physiological dysfunction. It should also prove useful to the physiologist as an aid in preparing "correlation" conferences and lectures.

PATHOLOGY IN GENERAL SURGERY.

By Paul W. Schafer. University of Chicago Press, Chicago. $17.50. xvi + 581 pp.; ill. 1950.

This volume is not the usual textbook of surgical pathology but is a survey of pathological lesions which may be found in specific sites and systems. In each area the author describes possible pathological changes in sequence of congenital defects, inflammatory lesions, and neoplasms. There are clinical correlations for each condition, and descriptions of gross and microscopic changes are further clarified by discussions of theories of pathogenesis. Sequelae and complications are described as well as therapeutic recommendations.

There are 368 remarkably well chosen color prints illustrating virtually every condition described, and many pertinent x-ray photographs are also reproduced. A satisfactory bibliography is appended to each section.

The material is covered topographically in 14 chapters, as follows: Parietes and general aspects; the vascular system (including lymphoid structures); thyroid, parathyroid, and adrenal; breast; lung and mediastinum; head and neck; esophagus, stomach, duodenum and small intestine; appendix, colon, and rectum; pancreas; liver; biliary system; and peritoneum.

This pathological survey is pertinent only to the general surgeon, since no attempt has been made to include neuropathology or the pathology of the urogenital systems. There is no mention of sarcoids, but with this single exception, this book provides a comprehensive survey of all pathological conditions that concern the general surgeon. In addition it is to be recommended to students and clinicians interested in topographical differential diagnosis.

ELLA H. OPPENDEIMER

ENDODONTIA. *The Clinical Pathology and Treatment of the Dental Pulp and Pulpless Teeth.*

By Edgar D. Coolidge. Lea & Febiger, Philadelphia. $6.00. 300 pp.; ill. 1950.

This is a beautifully organized book by a pioneer in the study and treatment of the dental pulp. The chapter sequence is logical and progressive, and each chapter is constructed so as to stand alone, having its proper introductory section, basic facts, discussion of instruments and materials, and directions for carrying out the recommended procedures. Each chapter is adequately illustrated—there are 179 figures in all—and has its own bibliography, including specific citations.

The presentation is so lucid and convincing that the reader who is not trained in endodontia may feel that "he too may be successful" if he follows the paths traced for him by the author. There is an adequate index but no summarizing list of authors cited.

JOHN A. CAMERON

PROGRESS IN CLINICAL ENDOCRINOLOGY.

Edited by Samuel Soskin. Grune & Stratton, New York. $10.00. xvi + 641 pp.; ill. 1950.

TEXTBOOK OF ENDOCRINOLOGY.

Edited by Robert H. Williams. W. B. Saunders Com-

pany, Philadelphia and London. $10.00. xii + 793 pp.; ill. 1950.

Soskin induced 91 men to contribute the 81 sections which comprise the 13 chapters of *Progress in Clinical Endocrinology*. That in itself is an accomplishment. The result is not what may be called a textbook of endocrinology, but a series of review articles that sum up the progress in some particular phase of endocrinology, with emphasis primarily on the pathological. This makes the book of especial interest to clinicians and practicing physicians. For the most part, the individual sections are authoritatively prepared and well written and will serve the purpose of apprising the physician of some of the rapid advances in clinical endocrinology.

On the other hand, the textbook edited by Williams with the collaboration of 11 other contributors represents the development of a textbook in the classical style. Here the subject matter is discussed from both the normal and pathological points of view and, on the whole, succeeds in providing a fairly condensed discussion of the management of clinical endocrinopathies as well as some of the fundamental chemical and physiological aspects of the subject. The book is well written. Most chapters have an adequate bibliography, and the whole should be of value as a general textbook of endocrinology for use by medical teachers and students.

DAVID B. TYLER

PRIMER OF ALLERGY. *3rd Edition.*

By Warren T. Vaughan; illustrated by John P. Tillery; 3rd edition revised by J. Harvey Black. The C. V. Mosby Co., St. Louis. $3.50. 175 pp.; ill. 1950.

When Warren T. Vaughan first viewed his *Primer of Allergy* as it came from the press in 1939, he could not have imagined that it would have become so useful and so popular that it would appear in three editions, with three reprintings of the second edition. The third edition, published since Vaughan's death, has been made possible through the experienced and sympathetic aid of Harvey Black, his friend and colleague. Those familiar with the preceding editions will find that Black has performed his task well, and has made only the changes required by the accumulation of newer ideas in allergy and later methods. The major portion of the book remains unaltered.

The purpose of this volume was to provide information to the patient contending with an allergic ailment. Vaughan correctly assumed that to obtain the greatest success in treating allergic diseases each patient must be educated in his specific problem, and that intelligent cooperation is essential. In the first chapters he set forth in simple language the mechanisms of allergy and its several manifestations in respiratory, gastrointestinal, neural, and dermal forms. The symptoms of hayfever, asthma, gastroenteritis, migraine, urticaria, etc., are described and explained. The various airborne and food allergens are listed, discussed, and evaluated. Diagnostic methods are described by which exciting agents in the home and in the diet are discovered.

Once the specific etiologic agents are identified, steps to avoid them are necessary or where this is impossible plans for building up a patient's specific protective powers are essential. These procedures are described. Included are general directions for patients with hayfever, with asthma, allergic eczema, allergic headache, allergic colitis, or indigestion. Various drugs and medications are described, including the antihistaminics. The value of adrenocorticotrophic hormone and cortisone are not discussed, however, presumably because they were not known at the time the volume was published. Detailed advice is furnished upon the avoidance of specific food factors in the diet, such as egg, milk, or wheat. This volume remains an important contribution to the subject of allergy.

W. C. SPAIN

ANGINA PECTORIS AND MYCARDIAL INFARCTION *With Special Reference to the Autonomic Nervous System.*

By Heymen R. Miller. Grune & Stratton, New York. $8.75. xiii + 336 pp.; ill. 1950.

The physiopathologic basis, character, distribution, and innervation of cardiac pain are only partly agreed upon. The problem in its many ramifications is meticulously presented by Miller in an enlargement of an earlier, basically anatomical volume on autonomic regulation published in 1939. The insensitivity of the heart to direct trauma renders all the more impressive its usual marked reaction to even brief myocardial ischemia, with apperception of severe pain by the individual. The character of the pain is unlike that produced by peripheral stimulation, and the strong psychic reverberations and generalized autonomic reactions that commonly accompany it give anginal pain its individuality. Afferent fibers from the heart generally traverse the first 4 thoracic sympathetic ganglia and reach the reticular substance of the cord via the white rami communicantes and the dorsal ganglia of the corresponding spinal nerves. This intimate association of visceral and somatic afferents is believed to account for the fundamentally peripheral dermatomic radiation of anginal pain.

The anatomic material in the book is presented in a large series of excellent drawings of increasing complexity representing the sympathetic and parasympathetic systems, including accessory pathways and their relationships. The sinistral representation of cardiac pain, its psychological aspects, and the more unusual cervical, cranial, and abdominal areas of reference are thoroughly discussed. The presumed absence of afferent fibres in the cranial sympathetic nerves accomodates the introduc-

tion of a rather controversial concept of referral of pain by reflex over sympathetic efferent fibres in a manner analogous to the axon reflex. Clinical aspects of cardiac pain, including medical and surgical methods of treatment, are well covered. The world literature is comprehensively reviewed, although papers of recent years have been somewhat neglected. Despite considerable repetition and a rather difficult and heavy style this is an important work that should be known to teachers and serious students at all levels of medical education.

E. Converse Peirce, 2nd

Hypertension. *A Symposium Held at the University of Minnesota on September 18, 19, and 20, 1950, in Honor of Elexious T. Bell, Benjamin J. Clawson, and George E. Fahr.*

Edited by E. T. Bell. University of Minnesota Press, Minneapolis. $7.50. xii + 573 pp.; ill. 1951.

Cardiovascular disease has become Public Enemy Number One, and the particular symptom named hypertension is an especially deadly weapon. In these proceedings, 30 papers, together with related questions and discussion, are published. A very up-to-date bibliography accompanies each paper.

All aspects of the field have been covered by 24 authorities—research, theory, medical handling, surgery, etc. In all respects the volume is beautifully done, and will have lasting value to many workers in various branches of biology and medicine.

R. G. Grenell

Selected Studies on Arteriosclerosis.

By Rudolf Altschul. Charles C Thomas, Springfield, Ill. $5.50. xiv + 182 pp.; ill. 1950.

This monograph deals with both human and experimental cholesterol arteriosclerosis and is principally concerned with the histopathological observations of the author, who insists that the morphological analysis of arteriosclerosis is not by any means complete. The aims in this study were to emphasize some of the unsolved and overlooked morphological problems and to analyze and correlate the cellular pathology in the various angiopathies. Data on the amitotic dedifferentiation or regression of the endothelium to the mesenchymal stage is reported for the human proliferative as well as for experimental arteriosclerosis. Attention is given to species differences, to the effects of various cholesterol derivatives, the methods of administration, exercising, and various supplementary procedures on the course of experimental cholesterol arteriosclerosis. This monograph will be of interest to the specialist in this field.

David B. Tyler

Thromboembolic Conditions and Their Treatment with Anticoagulants.

By Charles D. Marple and Irving S. Wright. Charles C Thomas, Springfield, Ill. $8.50. xiv + 418 pp.; ill. 1950.

This monograph is essentially an authoritative critical digest of the important articles on thrombosis and embolism which appeared up to October, 1949. The topics discussed include the factors leading to thrombosis, the clinical conditions in which this occurs, the rationale for the use of anticoagulants, the technic of administration of heparin, dicumarol, and tromexan, and the therapeutic results obtained. Stress is laid on the dangers involved and the precautions necessary to avoid them. The discussions, based on the authors' extensive experience, serve to give a coherent, clear presentation of this difficult subject. The bibliography includes 684 titles, and there is an adequate index. An appendix includes detailed directions for determining prothrombin time by the 3 methods most commonly employed.

This book is in no sense a laboratory manual nor a treatise on diagnosis. It is written primarily for clinicians who wish to utilize anticoagulants intelligently in their practice. For such it will be very valuable and is highly recommended. It also furnishes a useful background for any who may wish to pursue more extensive studies of the subject.

P. W. Clough

Renal Diseases. *Second Edition, thoroughly revised.*

By E. T. Bell. Lea & Febiger, Philadelphia. $8.00. 448 pp.; ill. 1950.

In this second edition of his monograph, Bell has renewed his attempt to relate structural and functional changes in diseases of the kidney. His approach is that of the pathologist who looks at stained sections under the microscope, rather than that of the functionalist who studies clearances. Of renal physiology there is little; of physiological chemistry there is practically none. But the histopathological changes which occur in the many types of kidney disease are carefully and meticulously described, the illustrations profuse and beautifully printed.

The organization of the subject matter has been left as in the first edition: classification of renal disease, normal histology and physiology, pathological physiology, developmental anomalies, hydronephrosis, glomerulonephritis, diseases of the tubules, pyelonephritis, diseases of blood vessels, kidney diseases and metabolic disorders, and, finally, tumors of the kidney.

Though a vast amount of descriptive clinical material has been included, it is unfortunate that it was necessary to rely in most of the cases on semi-quantitative estimates of renal function. For example, proteinuria is reported on a 1 to 4 plus scale. The phenolsulfone-

phthalein test is used as a measure of renal function in a few cases; but more often only the blood urea nitrogen or creatinine concentrations are given. Little mention is made of the electrolyte disturbances which play such a dominant role in the late states of glomerulonephritis. It is distressing to find a statement (p. 33) to the effect that the tubules form ammonia from urea.

The contents have been brought up to date by the inclusion of recent work, though there are some omissions, particularly of articles which emphasize clearance studies. In spite of its limitations this book will be of particular value for the description of the changes found in the structure of the kidney after autopsy.

F. P. CHINARD

MEDICAL DISEASES OF THE KIDNEY. *An Atlas and Introduction.*

By J. F. A. McManus. Lea & Febiger, Philadelphia. $6.00. 176 pp.; ill. 1950.

This short volume is an excellent combination of monograph and atlas of the pathological changes in "medical" diseases of the kidney. The author first provides a thorough description of normal renal physiology and structure and gives a preliminary account of the reactions to injury that may occur in each unit of renal structure; that is, in each portion of the nephron, vessels, and interstitial tissue.

The main portion of the monograph is divided into two parts, on acute renal failure and chronic renal failure, respectively. The first category includes pathological pictures of acute glomerulonephritis, the crush kidney, acute hypertension and eclampsia, acute pyelonephritis, the toxic nephroses, and the acute termination of chronic renal failure. In the category of chronic renal failure, there are descriptions of chronic pyelonephritis, chronic glomerulonephritis, intercapillary glomerulosclerosis and polycystic kidney, as well as short summaries of the incidental lesions of chronic hypertension, diabetes, amyloid disease, lupus erythematosus, and syphilis.

Throughout all the portions of this book, there are good clinical correlations with the pathological changes. There is also a well-chosen bibliography. The greatest praise, however, should be showered on the 100 illustrations. Instead of illustrating microscopic changes in the kidney with routine hematoxylin and eosin stains, McManus has provided beautiful photographs of the periodic acid-Schiff staining of tissue, as well as by other special stains, to demonstrate changes invisible in routine preparations. These truly remarkable illustrations place this volume in a class of its own and make it a complete and excellent atlas of the pathology of the so-called "medical" diseases of the kidney.

ELLA H. OPPENHEIMER

BLINDNESS. *Modern Approaches to the Unseen Environment.*

Edited by Paul A. Zahl. Princeton University Press, Princeton. $7.50. xvi + 576 pp. + 15 pl.; text ill. 1950.

This is the first book to deal systematically with the problems of the blind. In it 31 experts (10 of them blind), brought together by invitation of the National Research Council under the auspices of the Veterans Administration, write about various aspects of adjusting the blind to modern life. There are 34 chapters grouped under the following major headings: History and Welfare; Education and Psychology; Vocational Considerations; The Military Blinded; Time-Tested Boons to the Blind (Braille, guide dog, talking book, etc.); Exploration of New Technological Horizons; and Ophthalmological Factors in Blindness.

A. CHAPANIS

THE ETHICAL BASIS OF MEDICAL PRACTICE.

By Willard L. Sperry, with a foreword by J. Howard Means. Paul B. Hoeber, Medical Book Department of Harper & Brothers, New York. $2.50. 185 pp. 1950.

This is a quite readable, learned, and intelligent book, discussing mostly ethics in general. As far as medical ethics in particular are concerned, it ignores (or does not know) most of the basic issues and the contradictory, sometimes sordid realities which force the medical profession continually to discuss ethical problems. It deals only with two concrete medico-ethical problems of importance: telling the patient the truth; and euthanasia. While I regret the author's lack of realism, I would be a hypocrite were I to criticize the absence of consistency and definiteness in his answers, for I could do no better. Only, for that reason, I would not write a book on it either.

ERWIN H. ACKERKNECHT

PSYCHOLOGY AND ANIMAL BEHAVIOR

PATTERNS OF SEXUAL BEHAVIOR.

By Clellan S. Ford and Frank A. Beach; foreword by Robert Latou Dickinson. Harper & Bros. and Paul B. Hoeber, New York; Eyre & Spottiswoode, London. $4.50. x + 307 pp.; ill. 1951.

Clearly and interestingly written, *Patterns of Sexual Behavior* presents the results of a comprehensive study of the sexual behavior of the Eutheria. Although the authors have been most successful in demonstrating how important a broad scientific study of these problems can be, this has not prevented them from presenting their material in such a way that it will be clear,

valuable, and useful to everyone. They have amassed and studied an enormous amount of data: material relative to the stimulation and excitation of the sexual organs, i.e., the sexual behavior of 190 human societies as well as various groups of animals. Their analysis is threefold: cross-cultural, evolutionary, and physiological.

The book is introduced by a section on The Task and the Methods, followed by chapters devoted to the nature of coitus, circumstances of intercourse, stimulation associated with coitus, attracting a sex partner, sexual partnerships, homosexual behavior, sexual relationships between different species, self stimulation and spontaneous orgasm, individual development of sexual behavior, fertility cycles, maturity and old age, and finally human sexual behavior in perspective. This final section is a most important document in itself. The authors state that they wished "to describe the basic sexual patterns of human beings and their closest (mammalian) relatives." They have accomplished a great deal more. By having examined the data in their cross-cultural aspects, the authors have shown us that the sexual behavior patterns among human groups are of all types. Different groups reveal the presence or absence of specific sexual practices and attitudes.

A most important contribution of the study is the conclusion of the authors that a number of patterns of sexual behavior are affected by evolutionary factors. They feel that these factors must be appreciated in any analysis of sexual behavior, and that they may be responsible for inherent biological tendencies toward certain activities. In discussing the cross-species, cross-cultural data available for study and interpretation, the authors demonstrate that much more of this type of data must be obtained, so that future studies can be based on the most scientific approach possible. This is a highly intelligent book which it is to be hoped everyone will read.

R. G. GRENELL

HANDBOOK OF EXPERIMENTAL PSYCHOLOGY.

Edited by S. S. Stevens. John Wiley & Sons, New York; Chapman & Hall, London. $15.00. xii + 1436 pp.; ill. 1951.

This extraordinary volume is unquestionably the most important single reference work in psychology to have been published during the last 10 or more years. In it, 34 contributors have attempted to systematize, digest, and appraise the mid-century state of experimental psychology. The range of topics covered is so great that the only adequate way of describing the material in it is to cite the Table of Contents. There are 36 chapters arranged in 7 major divisions as follows (the first chapter standing alone):

1. Mathematics, measurement, and psychophysics (S. S. Stevens).

A. Physiological mechanisms
   2. Excitation and conduction in the neuron (Frank Brink, Jr.).
   3. Synaptic mechanisms (Frank Brink, Jr.).
   4. Sensory mechanisms (T. C. Ruch).
   5. Motor systems (T. C. Ruch).
   6. Homeostasis (Edward W. Dempsey).

B. Growth and development
   7. Mechanisms of neural maturation (R. W. Sperry).
   8. Ontogenetic development (Leonard Carmichael).
   9. The genetics of behavior (Calvin S. Hall).
   10. Growth curves (Nathan W. Shock).
   11. Phylogenetic comparison (Henry W. Nissen).

C. Motivation
   12. Instinctive behavior: reproductive activities (Frank A. Beach).
   13. Learnable drives and rewards (Neal E. Miller).
   14. Emotion (Donald B. Lindsley).

D. Learning and adjustment
   15. Methods and procedures in the study of learning (Ernest R. Hilgard).
   16. Animal studies of learning (W. J. Brogden).
   17. Human learning and retention (Carl I. Hovland).
   18. Theoretical interpretations of learning (Kenneth W. Spence).
   19. Cognitive processes (Robert Leeper).
   20. The psychophysiology of learning (Clifford T. Morgan).
   21. Speech and language (George A. Miller).

E. Sensory processes
   22. Basic correlates of the visual stimulus (Deane B. Judd).
   23. Visual perception (C. H. Graham).
   24. The psychophysiology of vision (S. Howard Bartley).
   25. Basic correlates of the auditory stimulus (J. C. R. Licklider).
   26. The perception of speech (J. C. R. Licklider and George A. Miller).
   27. The mechanical properties of the ear (Georg von Békésy and Walter A. Rosenblith).
   28. Psychophysiology of hearing and deafness (Hallowell Davis).
   29. Taste and smell (Carl Pfoffmann).
   30. Somesthesis (William Leroy Jenkins).
   31. Vestibular functions (G. R. Wendt).
   32. Time perception (Herbert Woodrow).

F. Human performance
   33. Selection (Harold P. Bechtoldt).
   34. Training. (Dael Wolfe).
   35. Engineering psychology and equipment design (Paul M. Fitts).
   36. Work and motor performance (Robert H. Seashore).

Those persons who think of psychology as primarily concerned with behavior pathology may perhaps be surprised at the physiological flavor of these contents. This book, however, represents the hard core of experimental psychology, much of which is indeed physiological. Several of the chapters, as a matter of fact, were prepared by specialists who are not psychologists (Brink, Ruch, Dempsey, Sperry, Shock, Judd, von Békésy, Rosenblith, and Davis). At any rate, biological scientists will find much of the material in this volume of direct interest to them.

In other respects, this is an exceedingly difficult book to review. It contains many of the faults and advantages of handbooks. Taken individually, the contributions are scholarly, authoritative, and comprehensive. For example, Judd's chapter on the visual stimulus is probably the best of its kind anywhere. The 4 chapters on the auditory sense are a well-integrated quartet, solidly packed with an extraordinary amount of data. And so on. The chief fault of the volume is that in the entirety it lacks cohesion. There is some duplication of material in different chapters, and cross-references are too few. This is a common fault of handbooks, however, and considering the excellence of this one, it is a weakness easy to overlook. Here is a book which should be in every psychologist's library, and in the library of many biological scientists as well.

A. CHAPANIS

BEGINNING EXPERIMENTAL PSYCHOLOGY. *McGraw-Hill Publications in Psychology.*

By S. Howard Bartley. McGraw-Hill Book Co., New York, Toronto, and London. $4.00. viii + 483 pp.; ill. 1950.

Several different varieties of book have been written in the name of Experimental Psychology. First (historically, if not logically) is the out-and-out manual, the cookbook approach to the laboratory: "How To Use Some Psychological Equipment"—a convenient aid in arranging formal exercises, but only part of the story. Then there is the one that might well be called "Results of Many Experiments in Psychology." Here we find listed in detail, handbook style, the outcome of nearly every experiment bearing on selected variables of behavior—obviously not the text to guide the college sophomore toward an appreciation of the whys and wherefores of experimental method. Third, we have "Conclusions Drawn from Some Experiments in Psychology." This is the attempt to protect the student from the threatened intellectual indigestion of this rich technical diet by feeding him a thoroughly predigested and carefully strained gruel of generalizations centering around a similar list of variables. Thin fare for building psychological sinews! Once or twice, recognizing the omnipresent goal of all college courses in science, the attempt has been made to provide the student with the atmosphere and basic ingredients that will foster growth toward a real understanding of the rationale and logic of experiment. "The Logic of Experimental Science as Revealed in Psychology" is apparently a formidable pedagogical project.

As usual, the principal fault of categorizing is that no case quite fits the listed categories. There are only mixtures. But if the items noted above are accepted at least as suggesting extreme trends, a final one may be added, represented more or less by Bartley's textbook. To this we might apply the descriptive subtitle, "How 50 Experiments Have Been Done by Psychologists." Some readers may be moved to add the phrase, "Especially on Vision."

The plan of the book is the necessary result of a number of definite decisions by the writer. (1) "Every phase of psychology is potentially a part of experimental psychology . . . there is a way of doing genuine experimental work in each field" (p. 443). Further, a textbook in experimental psychology should represent all the major areas. (2) Yet, whatever actual investigations are considered should be described adequately, in contrast to the usually cursory presentation of techniques or findings. (3) At the same time, the book should deal "with principles by way of illustration rather than being a compendium of the findings of experimentation" (p. v). (4) Finally, the material should be arranged so as to fit conveniently into the assignment schedule of a 1-semester undergraduate course. *Beginning Experimental Psychology*, therefore, consists of 48 8- to 15-page chapters, one for each class-day. The first seven are devoted to the history and general methodology of experimental psychology, and to some psychophysical methods. There follow ten general sections: The Broad Features of Sensory Experience (17 chapters); Physiological Psychology (5 chapters); Comparative Psychology (4 chapters); Learning and Memory (4 chapters); Preparedness and Adequacy (3 chapters); Social Psychology (3 chapters); Legal Psychology (1 chapter); Child Psychology (1 chapter); Clinical Psychology (2 chapters); and Industrial Psychology (1 chapter). Typically, each of these chapters describes at some length the background, the procedure, the results, and the conclusions of a single "exemplary" experiment with ten essay questions for self-testing.

The success of this textbook will undoubtedly depend upon the supplementation provided by the instructor. The student is advised by Bartley to consider the individual experiments "in the light of reasoning and procedures set forth in the early chapters on the methods and logic of experimentation . . ." and is urged to "inquire into the investigators' purposes and how his investigation fits into the area which the work represents" (p. 3). But the "methodology" chapters are incomplete, and explicit cross-references to their content in the "experimental" chapters are few and far between. It is extremely dubious that the average undergraduate can bridge the gap of generalization unaided or grasp

the full implications of the illustrations. And much of the writing, it is feared, will be obscure to the uninitiated.

But it is probably asking too much for a textbook writer to do all the teaching. It is likely that the most satisfactory training in both the theory and the techniques of experimental psychology must be an individually tailored pattern combining reading, listening, and manipulating. Belief in this position may account for the diversity of plan among similarly titled textbooks.

FRANK W. FINGER

PSYCHOLOGY APPLIED TO LIFE AND WORK. *Second Edition.*

By Harry Walker Hepner. Prentice-Hall, New York. $4.75. viii + 711 pp.; ill. 1950.

This, the second edition of Hepner's popular textbook, has been considerably revised. The contents are grouped into 4 main sections: Understanding the Individual's Behavior Through the Adjustment Concept; Personal Problems; Industrial Psychology; and The Consumer. This is not in fact an introductory textbook in general psychology, but rather in certain aspects of applied psychology. As such, however, it is a good one.

A. CHAPANIS

COLLEGE PSYCHOLOGY.

By Warner Brown and Howard C. Gilhousen. Prentice-Hall, New York. $3.75. xii + 507 pp.; ill. 1950.

Written for the one-semester introductory course, *College Psychology* represents a stringent experimental treatment of the conventional topics. Illustrative material is drawn largely from the animal experimental literature, and numerous graphs and charts throughout the text testify to the rigorous scientific intent of the authors. Exception is taken to the newer concepts of "dynamic psychology," and various arguments are advanced to disprove their value. Somewhat in keeping with the over-all tone of the book is its fidelity to the earlier experimental literature, most of its references being made to experiments carried out between 10 and 30 years ago. The authors remark that "we have often deliberately chosen from studies as far back in the literature as we could go in order to convey the truth that psychology has historical foundations."

Because of the rather limited range of topics taken up in this work and the somewhat specialized treatment of them, this textbook does not present a serious challenge to those already popular in the general introductory course.

ERLING ENG

PSYCHOLOGY.

By J. A. C. Brown. Paul Elek, London. 8s. 6d. 141 pp.; ill. 1950.

A little book intended to be a layman's Baedeker on set topics in psychology. In a casual, chatty fashion everything from the Anatomy of the Nervous System through Social Psychology is swiftly, briefly, and broadly skimmed. Because of its tempo and brevity, oversimplifications and expressions of bias are not lacking. The following is an example of the latter:

> "... tests carried out in the United States during the First World War showed that the average I.Q. of negroes all over the country was far below that of white men. When, however, the results were divided between Northern and Southern States, it was found that while the average I.Q. in the South was 75, in the North it was as high as 104.7, or slightly above that of white men in the same areas. This might seem a perfect example of the effect of environment on intellectual ability, but, in fact, it is not. The probable explanation is that the more intelligent negroes are ambitious and tend to make their way to the North where conditions are better, while the less intelligent remain in the South."

This conclusion, of course, has been disproved by the work of Klineberg and others on negro intelligence and cultural environment, as well as by the work of Davis on the effect of cultural learning on intelligence test scores.

Despite such occasional lapses, the general character of the book is one of informed intelligence, and it should be of some interest to a number of casual readers.

ERLING ENG

THEORY OF MENTAL TESTS. *Wiley Publications in Psychology.*

By Harold Gulliksen. John Wiley & Sons, New York; Chapman & Hall, London. $6.00. xx + 486 pp. 1950.

Gulliksen has done an excellent job in bringing together in this volume the widely scattered literature and contributions to test theory. The fundamental assumptions and statistical basis of mental test construction are set forth in a systematic lucid manner. Derivations of formula are clear and complete enough to be easily followed.

The comprehensiveness of the book is best illustrated by the chapter headings: Basic Equations Derived from Definition of Random Error; Fundamental Equations Derived from Definition of True Score; Errors of Measurement; Effect of Doubling Test Length on Other Test Parameters; Effect of Test Length on Mean and Variance (General Case); Effect of Test Length on Re-

liability (General Case); Effect of Test Length on Validity (General Case); Effect of Group Heterogeneity on Test Reliability; Effect of Group Heterogeneity on Validity (Bivariate Case); Correction for Univariate Selection in the Three-Variable Case; Correction for Multivariate Selection in the General Case; A Statistical Criterion for Parallel Tests; Experimental Methods of Obtaining Test Reliability; Reliability Estimated from Item Homogeneity; Speed versus Power Tests; Methods of Scoring Tests; Methods of Standardizing and Equating Test Scores; Problems of Weighting and Differential Prediction; and Item Analysis.

Each chapter is followed by well selected problems, designed to give the student competence and increased understanding of the chapter's material. Answers to the questions are provided in the appendix. In addition the appendices contain: equations from algebra, analytic geometry, and statistics used in test theory; ordinates and areas of the normal curve; questions in statistics for use as a review at the beginning of the course; and examination questions on the content of the course.

The book is designed as a textbook for courses in test construction and theory. It presupposes, in general, a knowledge of elementary algebra and statistics on the part of the student. A few sections require a knowledge of the analysis of variance, matrix theory, or elementary calculus, but these sections are short and can be omitted without serious loss in continuity.

C. W. ERIKSEN

PERSONALITY—DEVELOPMENT AND ASSESSMENT.

By Charles M. Harsh and H. G. Schrickel. The Ronald Press Company, New York. $5.00. x + 518 pp. 1950.

This well organized compendium of research and theory in the fields of personality development and assessment is difficult reading merely because it contains so much material. It is an extremely objective and factual account of the study of personality. In many chapters the authors' comment is carefully separated from their presentation of the various points of view. The first half of the book discusses child development and development through adult life into old age. The second section is primarily concerned with theories of the adult personality, and ends with several chapters on the problems of personality assessment. There is an excellent bibliography which is kept sufficiently inconspicuous not to interfere with reading, yet is there when needed. All in all, this is one of the most scholarly discussions of the field of personality that has appeared recently. It is limited largely to reporting on the research and opinions of others and makes a relatively small original contribution to theories of personality.

ALFRED L. BALDWIN

THE INDIVIDUAL AND HIS RELIGION. *A Psychological Interpretation.*

Gordon W. Allport. The Macmillan Company, New York. $2.50. xii + 147 pp. 1950.

The chapters of this book are based upon the author's 1947 Lowell Lectures and take up the following topics: Origins of the Religious Quest; The Religion of Youth; The Religion of Maturity; Conscience and Mental Health; The Nature of Doubt; and The Nature of Faith.

As a psychologist who is well known for his emphasis on the "uniqueness" of the individual, it is not surprising to find Allport suggesting a definition of the "mature religious sentiment" as a "disposition, built up through experience, to respond favorably, and in certain habitual ways, to conceptual objects and principles that the individual regards as of ultimate importance in his own life, and as having to do with what he regards as permanent or central in the nature of things." The author makes clear that his present concern is "with the psychology, not with the psychopathology of religion," and his informed and appreciative references to rituals and beliefs from many world religions is evidence of his success in this direction.

As a thoughtful commentary by a venturesome psychologist on the role of religious values in human living the present book is to be recommended, particularly to those who believe that religious values must inevitably suffer when viewed in a psychological frame of reference. Readers expecting a more focused, theoretical treatment of the subject, however, are likely to be disappointed by what appears to be a partial surrender on the part of the author to the conceptual diffuseness of his subject. The resulting lack of clarity is further increased by the author's continual shuttling between a phenomenological position inside the religious believer's skin, and an external pragmatic position from which religious faith is judged in terms of its contribution to a "healthy personality." It is conceivable that a more ample statement could show that the two viewpoints are opposite sides of the same coin, but this the author has not done within the scope of his present modest series of essays

ERLING ENG

THE DEVELOPMENT OF REASONING IN CHILDREN WITH NORMAL AND DEFECTIVE HEARING. *Institute of Child Welfare Monograph No. 24.*

By Mildred C. Templin. University of Minnesota Press, Minneapolis. $3.00. xii + 143 pp.; ill. 1950.

This psychological monograph is, along with its sibling studies, competent, thorough, and cast in the rigidly stereotyped form of the genre which makes it, inevitably, rather pedantic.

Mildred Templin poses herself the research problem: how and to what extent do limitations of intrinsic versus extrinsic factors affect reasoning ability? The limiting intrinsic factor in her study is deafness of the individuals. The limiting extrinsic factor for comparison is the institutionalization of the young people, aged 10 to 20, who were studied. The author comes, by dint of involved and meticulous procedures, to the conclusion that restriction of environment due to the intrinsic factor, deafness, does affect the ability to reason, while the restriction of an individual's environment by institutionalization (the extrinsic factor) does not seem to impair reasoning ability. The ability to reason is demonstrated by the subject's performance in highly ingenious abstract tests. On the basis of this study, Templin points out a series of further research possibilities which might clarify or validate assumptions about the reasoning process in human beings.

HELEN ARTHUR

PSYCHOLOGY AND ART OF THE BLIND.

By G. Révész; translated from the German by H. A. Wolff. Longmans, Green and Co., London, New York, and Toronto. $8.50. xiv + 338 pp.; ill. 1950.

The title of this book is somewhat misleading. Actually, the first half of it is largely concerned with the psychological analysis of space and form perception as derived through the sense of touch—the science of haptics. The second half of the book deals with the aesthetics of form and the sculptural creativity of the blind. The author is a psychologist of repute and director of the Psychological Laboratory at the University of Amsterdam.

Although optics and acoustics are well-developed sciences, haptics has been largely ignored, and this book is a distinct contribution to the field. Unfortunately, the author is wordy and difficult to follow. In part, this appears to be due to a translation which follows the original German too closely. For all that, there is much new and interesting material here which the specialist in this area cannot afford to overlook.

A. CHAPANIS

PERSONALITY IN PEPTIC ULCER.

By Albert J. Sullivan and Thomas E. McKell. Charles C Thomas, Springfield, Ill. $3.00. x + 100 pp.; ill. 1950.

The authors and undoubtedly some others will not agree with the following diagnostic questionnaire. It was inspired by reviewing the evidence to date presented by those psychosomatically enthused.

Are you the driving type? Must you be driven? Are you versatile? Have you two left hands? Do you respond emotionally? Are your sexual problems conspicuous by their absence? Are you self-reliant? Do you require your wife's assistance to tie your bow? Your husband's help to hook your dress? Are you determined? Lackadaisical? Responsible? Irresponsible? Are you old? Young? Man? Woman? Black? White? Do you live in a city? A town? A village? On a farm? In Baltimore? Are you of low intelligence? Average? Superior? Yale graduate?

If your answers are yes or no to any of the above questions the chances are you may or may not get ulcers.

DAVID B. TYLER

PERSONALITY AND PSYCHOTHERAPY. *An Analysis in Terms of Learning, Thinking, and Culture.*

By John Dollard and Neal E. Miller. McGraw-Hill Book Co., New York, Toronto, and London. $6.50. xiv + 488 pp. 1950.

The basic concepts of this book are credited to Freud and Pavlov, though in reading it one is equally impressed with the use of concepts from the general field of semantics, to which the authors give scant credit. The principal concepts from Freud concern the transference situation. Extremely little is used from Freud's developmental psychology, and indeed the authors discredit it when they head a section Needed Too: A Science of Child Rearing. This brief section completely neglects the facts available in this field and makes no attempt to analyze them, brushing all aside with "fad after fad sweeps the field." Generally speaking, there is an almost complete disregard of the process of personality formation and maturation, and the psychogenesis of conflict is not at all thoroughly discussed. This gives rise to a narrow interpretation of cases which leaves one with a very distinct impression of superficiality. Much of the dynamics in the book concerns precipitating situations, not fundamental dynamics in the medical and psychiatric sense.

Although there is constant verbal obeisance to the idea of "free" association, one is startled at the frequent occurrence of very strong forcing of the patient into particular lines of thought which are in accord with the therapist's understanding of the case. It is hard, sometimes, to escape the feeling that the therapist acts like a benevolent tyrant who really knows all the answers and holds the well-fitted key to successful treatment. He must be skilful enough to grease the ideational (learning) skids so that the patient can slide home to health. This statement is certainly not what the authors meant to say, but there is in their words a startling neglect for what Meyer characterized as "spontaneity" in the patient. There is too much of mechanics and not enough of feeling and not enough respect for the complexity of the task of therapy.

In spite of these criticisms, this book is a valuable and stimulating one. Learning theory and experimental data are applied rigorously to a difficult area, that of psychotherapy, which is too often left unnecessarily vague and mystical. Students wishing to know "how to do it" will find too many easy answers here, but they will also find a firm skeleton around which to organize their thinking. This structure will need a great deal of filling in as they acquire experience in therapy, and that experience will show that it cannot incorporate much that they must learn, but a structure, even though not complete, offers great reassurance to the beginner.

PAUL V. LEMKAU

TEXTBOOK OF ABNORMAL PSYCHOLOGY. *Revised Edition.*

By Carney Landis and M. Marjorie Bolles. The Macmillan Co., New York. $5.00. xii + 634 pp.; ill. 1950.

This revision continues to be one of the best presentations of the subject I know of. The authors stamp themselves as eclectics and give a good presentation of the contributions of various viewpoints toward the subject-matter. It is, for instance, rather interesting to see here a considerable discussion of disorders of will, a matter which is quite well ignored in most textbooks of psychology. The book is divided into several sections: Orientation; Varieties of Abnormality; Explanations; Psychopathology; and Diagnosis and Therapy. The authors are quite right, I believe, in pointing out the differentiation between dynamics as explanation and dynamics as a description of the mental processes of individuals, and they are also right in stressing the real value of the latter and the dubious value of the former. There is an excellent glossary of psychiatric terms and an all-inclusive index. This is a book which can be thoroughly recommended.

WENDELL MUNCIE

MENTAL ABNORMALITY—*Facts and Theories.*

By Millais Culpin. Longmans, Green & Co., New York; Hutchinson's University Library, London. $1.60. 296 pp. 1950.

This rather readable and on the whole sensible portrayal of the development of modern psychiatry includes chapters on its historical development, and others on the actual description and psychopathology of various syndromes with short comments on their treatment. It is curious to find in a chapter on mental defect a subgroup dealing with psychopathological personalities, here referred to as temperamental defects. There is also a curious discussion of the makeup of Adolf Hitler with some diagnostic conclusions which I could not share. On the other hand, armchair speculation about the correct diagnosis of Hitler is not very important, and this criticism of the book is really of no consequence. The book is a welcome addition to the literature of modern psychiatry, useful to students, physicians generally, and, I should think, to intelligent laymen.

WENDELL MUNCIE

THE WRITER AND PSYCHOANALYSIS.

By Edmund Bergler. Doubleday & Co., Garden City, New York. $3.50. xx + 265 pp. 1950.

Bergler's latest book again illustrates his talent for expressing his ideas clearly, easily, and with good humor. The volume contains the author's views on the causes for the sterility of productivity in writers, as deduced from 36 cases which Bergler has analyzed for varying periods of time, generally 2 years or longer. The principal thesis which the author seems to propound is that a writer's inspiration, or lack of it, is attributable to his being "a perpetual defendant accused before the high tribunal of his unconscious conscience. What the writer unwillingly produces is nothing but an unconscious secondary alibi defense."

The particular value of this investigation seems to be the author's valiant attempt to create some order out of the chaos of supposition and amateur analysis indulged in by the general reading public in its attempt to understand artistic creation. The implications of such a study obviously go beyond a mere clinical treatment of authors themselves, but also attempts to establish a basis for understanding the problem of creativity in general. It seems to me that there can be little criticism of Bergler's success and ability in the clinical treatment of these described cases, although some of his conclusions may seem undocumented to many psychiatrists. Actually Bergler anticipates the principal criticisms of his book by suggesting in the Foreword that he will hear the following objections: "that I am unduly generalizing; that the writers I analyzed were 'half artists' . . .; and finally that I seem to harbor some hidden jealousy or animosity toward writers." The author attempts to refute each of these criticisms. In spite of this, however, one feels that the cases are perhaps too few in number to be used for a generalized theory. Bergler is himself a prolific writer and one widely read in psychiatric circles. It is possible that his experience in this field has given him better personal equipment to understand the problem of writers than the average psychiatrist.

ESS A. WHITE, JR.

CHRISTIANS AND JEWS. *A Psychoanalytic Study.*

By Rudolph M. Lowenstein; translated from the French by Vera Damman. International Universities Press,

New York; Presses Universitaires de France, Paris. $3.25. 224 pp. 1951.

Over 10 years ago, in France, the author became interested in anti-Semitism and began observing reactions in his patients. He found many types, "ranging from the group which merely thinks in emotional stereotypes or in terms of self-interest, through the more seriously contaminated group which has fixed delusions about the Jews, to the most dangerous group of all whose hostility towards Jews is translated into overt acts of criminal violence." Analyzing the origins of anti-Semitism, he notes the significance of ambivalence and the Oedipus complex. The same mechanisms operate whether anti-Semitism occurs at the individual or the collective level.

Four major motives for anti-Semitism are found: the political, xenophobic, economic, and religious factors. Consideration of "Jewish character traits" leads to the conclusion that they are the traits common to all human beings. But the consciousness of being Jewish invariably invokes a defensive attitude, "in response to an inner perturbation compounded of all the outrages ever perpetrated against the Jewish people. . . ."

Historically, says Loewenstein, Christianity and Judaism have become a "cultural pair"; the Jews are hated "because they have managed to survive through centuries of persecution, and to survive as a distinct group among the peoples who have both sheltered and persecuted them." He is not optimistic about the elimination of anti-Semitism, but he finds it reasonable to hope that mutual tolerance and cooperation will develop in the not too distant future. Loewenstein's scholarly and urbane approach to the problem, emphasizing dynamic factors but with awareness of social and cultural influences, is an outstanding positive contribution.

S. S. SARGENT

A GUIDE TO PSYCHIATRIC BOOKS *with a Suggested Basic Reading List. The Menninger Clinic Monograph Series No. 7.*

By Karl A. Menninger, with the collaboration of George Devereux. Grune & Stratton, New York. $3.50. viii + 148 pp. 1950.

This is a reading list of books considered important in the formal training of young psychiatrists, with some suggestions as to the more important books to be covered. The list covers neurology, psychology, anthropology, sociology, religion, psychiatry and psychoanalysis, mental hygiene, and even neurosurgery, as well as the adjunct therapies. It is a long list, fairly eclectic, and will be welcome in all training circles in the field of psychiatry. The list includes only books published prior to July 1, 1949.

WENDELL MUNCIE

HUMAN BIOLOGY

THE FIRST TWO DECADES OF LIFE. *A Revision and Extension of From Infancy to Adolescence.*

By Frieda Kiefer Merry and Ralph Vickers Merry. Harper & Bros., New York. $3.75. xvi + 600 pp.; ill. 1950.

This is a very complete textbook covering the various developmental areas of children's life from conception through adolescence. The topics cover physical, intellectual, social, and emotional aspects of the growing child's experience. The chapters are well arranged for a study program. The main facts and research data are given, with subtitles inserted frequently to divide the material into palatable amounts. At the end of each chapter is a summary, followed by a section on Suggested Activities and a list of references applying to the particular problem under consideration. Within the limitations imposed by the obligation to deal with such a mass of detailed facts, the authors have managed to hold to a clear, uncomplicated prose style which makes the book quite readable.

HELEN ARTHUR

THE HANDICAPPED CHILD. *A Guide for Parents.*

By Edith M. Stern with Elsa Castendyck. A. A. Wyn, New York. $2.00. xii + 179 pp. 1950.

Mrs. Stern and Miss Castendyck have written this useful, and therefore important, book as a guide for those troubled people who have a handicapped child. With sympathy but not sentimentality the authors have taken up in a straightforward manner the problems of the crippled child, the child with cerebral palsy, the epileptic, the blind child, the deaf child, the mentally retarded child, the child with speech defect, and the child with rheumatic fever or some other long illness.

Parents are given a clear positive evaluation of what the child must face. They are reminded of the difficulties the handicap imposes personally upon them as parents. And then they are offered concrete suggestions as to what they can do to help their child be a happy child. In each section the name and address of the National Agency which can offer specific help in the problem under consideration is given.

The whole spirit of *The Handicapped Child* is one of encouragement to parents to face difficult reality with fortitude and positive action. It is a book which ought to be made available to every parent who must experience the bringing up of an abnormal child.

HELEN ARTHUR

THE HUMAN USE OF HUMAN BEINGS. *Cybernetics and Society.*

By Norbert Wiener. Houghton Mifflin Co., Boston. $3.00. x + 241 pp. 1950.

In this book, the author of *Cybernetics* speculates about the impact which the new advances in electronics will have upon our society. The development of thinking machines—today's giant electronic brains—marks, he feels, the beginning of a new industrial revolution that will affect us all in one way or another. His inquiries take him into such diverse fields as communication behavior, language, law, secrecy, politics, communism, and catholicism. Although the reader may find the connections between some of these topics a bit tenuous, the book is well worth reading for its view of one man's careful analysis of exciting and dangerous things to come. Like its predecessor, this book is engagingly written. It is, moreover, less technical.

A. CHAPANIS

SOCIAL PSYCHOLOGY. *An Integrative Interpretation.*

By S. Stansfeld Sargent. The Ronald Press Co., New York. $4.50. x + 519 pp. 1950.

In this introductory textbook Sargent attempts to bring together the approaches to the study of social behavior characteristic of anthropology, psychiatry, psychology, and sociology. It is the author's opinion that the time is now ripe to integrate the contributions of each approach and to broaden the specialized viewpoints involved. Although his training and background are principally those of a psychologist, he is on the whole successful in achieving his purpose.

The text is divided into four parts, the first of which is concerned with the socialization of the individual. Here the social forces that affect personality are described; the interaction of biological and social factors in determining individual behavior, and socialization as a form of the learning process are considered. Part II, the least extensive section, centers about the dynamics of social behavior, motivation, frustration, and ego involvements. The patterning of social behavior is considered in the third part, consisting of the processes of communication, the forms of social interaction, the importance of roles in social behavior, leadership, and a field theory approach to the person in social situations. The fourth part is concerned with understanding social phenomena. It includes chapters on public opinion propaganda, mass behavior, social movements and change, prejudice, and the application of social psychological principles to the area of social issues, e.g., industrial relations and international conflicts.

While space does not permit a detailed critique of this book, one is impressed with its over-all advantages. The text is written in a straightforward, readable style; there are relatively few obscure passages; and each chapter ends with a concise summary. The instructor whose predilections are in the direction of the field theoretical and situational approaches to this subject will find this textbook a highly useful teaching aid. The organization of the material is adequate, although I would prefer a more unified treatment of certain important topics, especially that of attitudes. The amount of material included is not so comprehensive as in some recent longer textbook, and in general, the problems of methodology and measurement in conducting social psychological research are not emphasized. This is suitable for an introductory book, however. For longer courses or for more extensive treatment, Sargent has included at the end of each chapter a carefully selected list of up-to-date supplementary readings.

H. J. BOND

THE "WHY" OF MAN'S EXPERIENCE.

By Hadley Cantril. The Macmillan Co., New York. $2.75. xiv + 198 pp. + 5 pl. 1950.

It is disheartening to contemplate the fate of this excellent, sober, and serious effort to provide an interpretation of human behavior. No full-page ads in the *New York Times* or *Herald Tribune* attend its publication or distribution; and very likely this book, like many others that could exert a positive effect on its reader, will have an audience of a bare thousand or so who somehow, like myself, stumble on it. Meanwhile, the spurious *Peace of (Mind, Soul,* etc.) volumes are to be seen everywhere, and a pap-filled *The Mature Mind* wins prizes while it occupies a niche week after week on best-seller lists.

Cantril's aim in this small book is to attempt "to understand the nature of those transactional relationships . . . which account for what order and predictability there are in human life." In 8 well-tempered chapters he develops a consistent point of view which he has derived from many sources, notably the work of Ames and others on the nature of perception and his own findings from many years of original research in social psychology. Basic to the point of view is a proposition that undercuts, where it does not cut across, other fundamental hypotheses of psychological systems: namely, that an outstanding (if not *the* outstanding) "characteristic of man is his capacity to sense the value in the quality of his experience." It is this essential premise—derived scientifically—which leads Cantril to fixate as the "ultimate, the most generalized goal of man . . . what can be called the *Enhancement of the value attributes of experience.*" From this follow cogent and meaningful observations and proposals of a psychological nature which, in the end, add up to a comprehensive account of the nature of human nature. While I do not agree on all counts with Cantril's premises or his conclusions, I feel very strongly that the viewpoint deserves a wider hearing than it seems likely to get.

ROBERT LINDNER

BIOMETRY

CONTRIBUTIONS TO MATHEMATICAL STATISTICS. *Wiley Publications in Statistics.*

By R. A. Fisher; index prepared by John Tukey. John Wiley & Sons, New York; Chapman & Hall, London. $7.50. Unpaginated but numerically indexed reproductions of 43 original papers [ca. 750 pp.]; 1 pl. 1950.

Students of statistics probably consult original sources less frequently than students in other disciplines. In part, this may be due to the difficulty of locating the journals in which statistical articles appear. Only large university libraries are apt to have such diverse journals as *Biometrika*, *Metron*, *Sankhyā*, *The Annals of Eugenics*, *Journal of the Ministry of Agriculture*, *Economica*, and the *Proceedings of the Cambridge Philosophical Society*. For this reason, the present compilation of the works of R. A. Fisher should be of great service.

Fisher needs no introduction to biometricians and statisticians. His name is repeatedly associated with important contributions to such topics as small sampling techniques, fiducial probabilities, the analysis of variance, and the design of experiments. This volume contains 43 of his most important papers on these problems. Unlike many "collected works" published in memory of distinguished scholars, this collection has received the personal attention of the author. Although the articles are reproduced here exactly as they appeared in the original journals, Fisher has added a short introduction to each one to explain why it was written and what its contribution is, in the light of present statistical knowledge. In addition, the author has annotated each paper, correcting earlier mistakes, amplifying obscure sections, and adding new material. The net result of this editorial work is to increase greatly the value of these reprinted articles.

This, of course, is not an integrated work on statistics; it is a reference handbook. As such, however, it should be of great value to professional statisticians and biometricians.

A. CHAPANIS

CHANCE AND CHOICE BY CARDPACK AND CHESSBOARD. *An Introduction to Probability in Practice by Visual Aids. Vol. I.*

By Lancelot Hogben. Chanticleer Press, New York (printed in Great Britain). $12.50. 417 pp.; ill. 1950.

This, the first of a 2-volume set dealing with statistical theory, covers what may be called the classical treatment of large samples in both its aspects—the sampling of attributes and the sampling of measurements. There are 10 chapters, as follows: Figurate Numbers and Fundamental Approximations; The Calculus of Choice Hypothesis and Expectation; The Recognition of a Taxonomic Difference; Significance and Confidence; Interlude on the Method of Moments; The Recognition of a Mean Difference [for large samples]; Correlation and Independence; The Nature of Concomitant Variation; and Preview of Sampling Systems. The book is ingeniously illustrated with drawings showing various aspects of probability theory applied to familiar examples of card games and chess problems. Despite the pedagogical cleverness used by the author in presenting his material, this is a high-level text well beyond the average student in the biological sciences.

A. CHAPANIS

STATISTICS. *An Intermediate Text Book. Vol. II.*

By N. L. Johnson and H. Tetley. Cambridge University Press, New York and London. $4.00. xii + 318 pp. 1950.

This is the second volume of a 2-volume work, the first of which was reviewed earlier (*Q.R.B.*, 26: 122. 1951). The present work has 7 chapters dealing with the calculus of distribution functions, applications of the calculus of distribution functions, the multinomial distribution and its applications, general theory of statistical tests and estimation, stratified populations and the analysis of variance, correlation analysis, and curve fitting and graduation. Five appendices, totalling 25 pages, deal with the beta and gamma functions, multiple integrals, tables of the chi-square distribution, the t distribution, and the F distribution. The text is liberally supplied with examples and problems for which solutions are provided.

This book was written at the request of the British Councils of the Institute of Actuaries and the Faculty of Actuaries to assist in the training of their students. It develops the subject systematically, with proofs, and at a fairly complex mathematical level. Although this treatment has its commendable features, students and research workers in the biological sciences will probably find any of several other texts more suited to their needs.

A. CHAPANIS

FUNDAMENTAL STATISTICS IN PSYCHOLOGY AND EDUCATION. *Second Edition*

By J. P. Guilford. McGraw-Hill Book Co., New York. $5.00. xiv + 633 pp. 1950.

The second edition of Guilford's textbook has been

greatly revised and expanded. It has 5 more chapters and is almost exactly twice the length of the first edition. This new volume is a sound introduction to the fundamentals of statistics, but, as its title suggests, it will be useful primarily to students of psychology and education because of its heavy emphasis on test scales, norms, and related applications.

A. CHAPANIS

DE OMNIBUS REBUS ET QUIBUSDAM ALIIS

SCIENCE IN PROGRESS. *Seventh Series.*

Edited By George A. Baitsell. Yale University Press, New Haven. $6.00. xviii + 512 pp.; ill. 1951.

This latest volume in the series presents, like its predecessors, contributions from leading scientists in a wide variety of fields. The scope of Series VII can be appreciated from the chapter headings: The First Heart Beats and the Beginning of Embryonic Circulation (Bradley M. Patten); The Reproductive Cycle of the Rhesus Monkey (George W. Corner); Human Infancy and the Embryology of Behavior (Arnold Gesell); Radiation Damage to the Genetic Material (H. J. Muller); Beyond the Gene—Two Years Later (T. M. Sonneborn); The Macromolecular Structure of Biological Materials (Ralph W. G. Wyckoff); The Elementary Particles of Physics (Carl D. Anderson); Atomic and Solar Energy (Farrington Daniels); Atomic Structure and Energy (J. R. Dunning); The Theory of Braids (Emil Artin); History of the Fauna of Latin America (George Gaylord Simpson); The Physical Chemistry of Polymers (Raymond M. Fuoss). Like those before it this volume is an incomparable source of authoritative information. It deserves the attention of layman and scientist alike.

In comparing the subjects which have been discussed since 1937, when the series started, it is interesting to note that, of the 74 papers comprising the series, 11 have dealt with elementary particle and atomic physics, 6 with other aspects of physics, 8 with chemical topics, 7 with various aspects of genetics, 5 with vitamins and hormones, 5 with neurophysiology, 4 with geological subjects, 4 with astronomy, 3 with viruses, 4 with miscellaneous aspects of medical science. The remaining contributions represent a scattering of fields. Topics in behavior, embryology, psychology, and plant sciences, to mention only a few, are of rare occurrence. One is inclined to wonder whether the bulk of scientific effort is indeed being directed into the fields of atomic and nuclear physics and genetics which fields combined account for nearly 25% of the contributions) or whether these fields so outweigh others in importance as to deserve such emphasis. There is, of course, always the possibility that science is not progressing so rapidly in other areas.

V. G. DETHIER

SCIENCE NEWS 20.

Edited by A. W. Haslett. Penguin Books, Harmondsworth, Middlesex; Baltimore. 50 cents (paper). 128 pp. + 16 pl.; text ill. 1951.

Of biological interest: Advances in Anaesthesia (V. Keating); Control of Insect Populations (A. G. Thomson); A Definition of Man (Kenneth Oakley); The Origin of Language (R. A. S. Paget). Research Report: Bacterial Variants; Germ-free Mammals (A. W. Haslett).

SCIENCE NEWS 21.

Edited by A. W. Haslett. Penguin Books, Harmondsworth, Middlesex; Baltimore. 50 cents (paper). 126 pp. + 16 pl.; text ill. 1951.

Biological contributions to this number are as follows: Identical Twins in Dairy Cattle Improvement Investigations (John Hancock); Forest Entomology (J. J. de Gryse); Plant Nutrition and Disease (anonymous). In Research Report, A. W. Haslett discusses Virus Variations, Amino-Acid Synthesis, Body-Weight and Fat, and Oceanography.

GERMAN SCIENCE READER. *An Analytical Approach to Translation Problems.*

By George Wm. Radimersky. Ronald Press, New York. $3.00. viii + 245 pp. 1950.

This book is for the student who is beginning his second year of German and who is most interested in acquiring some proficiency in translating technical material. The text is divided into two main parts. The first consists of 10 chapters and problem readings, each of which singles out an important group of difficulties encountered by the student. In analyzing these difficulties primary emphasis is on the practical business of translation.

The second part of the book contains 12 essays in German for practice in translation. Certain expressions which are frequently found in technical German are singled out for special comment at the foot of the page below the various readings. Seven of the 12 essays deal with biological, or related, topics.

Three appendices contain technical hints and comments regarding certain important points in translation, a list of common technical phrases, and a list of technical abbreviations. There is a 28-page German-English vocabulary in the rear of the text.

This is the sort of book which should be of great help to graduate students who are trying to cram for their German language examinations.

A. CHAPANIS

MCCLUNG'S HANDBOOK OF MICROSCOPICAL TECHNIQUE *for Workers in Animal and Plant Tissues. Third Edition, revised and enlarged.*

By 35 authors; edited by Ruth McClung Jones. Paul B. Hoeber, Medical Book Department of Harper & Bros., New York. $12.00. xx + 790 pp.; ill. 1950.

The changes which have been made in this edition are mostly those necessary to bring the book up to date. New chapters, for instance, have been added on the phase microscope and its application to biological materials; the preparation of materials for electron microscopy; radioautographic technique; and the microscopical investigation of biological materials with polarized light. With the exception of the chapters on bacteriological and botanical procedures, which have been deleted from the present edition, most of the old sections have been retained but revised. Those on fluorescence microscopy, bone and cartilage, and embryology are larger and more comprehensive than previously. Easy to follow, graphic, representations of embedding schedules are included in the general section. The use of newer reagents in conventional techniques is discussed in appropriate places, and methods for embedding in plastic are described. This book continues to be among the best in the field of microscopic technique.

MARY E. RAWLES

THE MICROTOMIST'S VADE-MECUM (BOLLES LEE). *A Handbook of the Methods of Animal and Plant Microscopic Technique. Eleventh Edition.*

Edited by J. Bronte Gatenby and H. W. Beams. Blakiston Co., Philadelphia. $8.50. xiv + 753 pp. + 1 pl. 1950.

The Microtomist's Vade-Mecum was first published in 1885 by the distinguished investigator, Arthur Bolles Lee. Now in its eleventh edition, there are still parts which are largely his work. In the present edition the chapters have been arranged somewhat differently. Those on Tissue Culture and on Staining have been omitted, and a chapter on The Laboratory Cultivation of Invertebrata has been added. In general, the editors have tried to keep as much of the older material as possible, since many of the older methods have once again become useful. Some of the older names for chemical substances have been changed, also many of the latin terms so frequently used previously have been left out "to assist those who have had the benefits of a modern education." Other than notes on Zernicke's microscope, given in connection with the Golgi apparatus and mitochondria, optical instruments are not considered. This source-book of valuable and useful information now, as always, appeals rather to the experienced worker than to the beginner. Few books have maintained their value and excellence throughout a comparable period of time. The *Vade-Mecum* is still unrivalled in its field.

MARY E. RAWLES

MAN'S PHYSICAL UNIVERSE. *A Survey of Physical Science for Colleges. Third Edition.*

By Arthur Talbot Bawden. The Macmillan Co., New York. $4.75. xvi + 822 pp.; ill. 1950.

This very comprehensive survey of the physical sciences emphasizes the applications of scientific discovery and the current status of technology. It is planned for the beginning college student, and should interest him, although the pages present a rather cluttered and cut-up appearance because of the use of headings and the abundance of illustrations. In some sections the relationships of the physical to the biological sciences are slighted. For example, in the unit on geology, although soil formation and erosion are treated briefly, there is practically nothing from paleontology. Again, in discussing light and sound, there is insufficient consideration of eye and ear, of vision and hearing. On the other hand, an entire final unit is devoted to such biological topics as the derivation of food from photosynthesis; the importance of vitamins, enzymes, and hormones; and chemical insecticides, drugs, anesthetics, and antibiotics. Only too often the author fails to avoid the imminent danger that threatens all such surveys. The flood of information overwhelms the poor seeker after understanding. The text becomes an encyclopedia, then degenerates to a mere catalog of items.

Unit I is devoted to scientific attitudes and methods. It is wholly didactic in character, and judging from experience, I would say is entirely unlikely to inform any young college student what science is really like. In many colleges books such as this are used for courses without laboratory work, the lack of which further divorces the student from real contact with observation, measurement, hypothesis and verification. In such a situation, surely the best substitute for personal scientific experience is the vicarious one which might be supplied by the case study method so ardently espoused by President Conant of Harvard University. At least it would add body to a section of *Man's Physical Universe* which at present reads like the professional educator's analysis of scientific method, right out of a teachers' college.

BENTLEY GLASS

PATTERNS FOR LIVING. *Alternate Edition, Part I and Part II.*

Edited by Oscar James Campbell, Justine Van Gundy, and Caroline Shrodes. The Macmillan Company, New York. Vol. I $3.75; Vol. II $3.75. Vol. I: xviii + 878 pp. Vol. II: xvi + pp. 673–1302. 1947.

In these 2 volumes of excellently chosen readings for the young student, there is one section (150 pages) devoted to the World of Science. It includes 7 essays: The Struggle for Existence (Charles Darwin); Is Man an Absurdity? (John Hodgdon Bradley); Modern Man is Obsolete (Norman Cousins); When H. G. Wells Split

the Atom (Freda Kirchwey); Apology for Man (Ernest A Hooton); Breaking the Vicious Circle (Karl Menninger); and Sex ex Machina (James Thurber). All are well worth reading, if in some cases over-condensed or taken out of context.

Biography is represented by 3 selections: The High Plateaux of Asia (Oliver La Farge); Four Years in a Shed (Eve Curie); and The Campers at Kitty Hawk (John Dos Passos). Each of these has been taken from a book of notable stature. Each is therefore episodic; but within their limits they manage to convey in a striking way something of the unquenchable spirit of science. Each is by a master of prose, yet each is altogether different in quality and movement. A section of fiction offers the reader first a selection from *Arrowsmith*, by Sinclair Lewis, called In a College Laboratory; and second, a short story by Edith Wharton—The Debt. These continue the exploration of the scientific spirit and the personality of the scientist—the intoxication of the young scientist in the work of discovery; the loyalty to the ultimate truth that sets aside the most cherished theories or personal loyalties. These selections are followed by a full length play, Karel Capek's "R. U. R."—a bit of science fiction transposed into dramatic form—a modern satire to stand beside *Gulliver's Travels* and *Erewhon*. The section ends with 6 short poems, by Morris Bishop, Carl Sandburg, Stephen Spender (2), Aldous Huxley, and Alfred Tennyson. These expressively convey the philosophical, creative, and impressionistic reactions toward a world of science.

The remainder of these volumes appears to be of the same high merit as the portion dedicated to science. The selections have been chosen with imagination and insight. Of anthologies there is indeed no end, and they can never replace a deeper acquaintance with the treasures of literature, scientific or otherwise. But if we must have anthologies—and everyone must enter each new field through some portal—then by all means let us have anthologies, like this one, that catch the spirit of the human enterprise before they overwhelm us with its facts.

BENTLEY GLASS

THE QUARTERLY REVIEW OF BIOLOGY publishes critical reviews of recent researches in all of the special fields of biological science. The contribution should present a synthesis or digest of the researches and a critical evaluation of them. A mere synopsis of the literature without evaluation or synthesis is not desirable.

Theoretical papers are published occasionally, especially when such papers (1) include a critical synthesis of the literature bearing on the theory and (2) are likely to promote further research in a given field.

The article should be written in concise language, yet in sufficiently non-technical form as to be intelligible not only to specialists in other fields but to the general biologist as well. To this end the article should have a general introduction and a summary which enumerates one by one all of the principal facts and conclusions given in the paper. Interpretative diagrams and schemes are very desirable.

Material ordinarily taking the form of footnotes is set in small print and placed in the text and consequently should be written in a style so as to fit readily into the text. Acknowledgments are printed in the text in small type at the end of the article just preceding the List of Literature. Recent issues of the Quarterly should be examined for style as regards (1) section or subsection headings in the text, (2) literature citations in the text, and (3) List of Literature.

The subjects and authors of articles are selected by the Editors and members of the Advisory Board. Unsolicited articles which conform with the objectives of the Quarterly will be considered for publication.

A feature of the REVIEW is the section dealing with *New Biological Books*. In this department the book literature of different countries in the field of Biology is given prompt notice.

The QUARTERLY REVIEW OF BIOLOGY is issued in March, June, September and December.

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Manuscripts may be sent to Dr. B. H. Willier, Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland.

Books for Review may be sent to Dr. H. B. Glass, Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland.

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